
A DEO ET REGE

AGRICULTURAL RESEARCH INSTITUTE

PUSA

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LVII. PART I. For the Year 1767.

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M.DCC.LXVIII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

 should

ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of nature or art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons, through whoſe hands they receive them, are to be conſidered in no other light, than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

CON-

CONTENTS

TO

VOL. LVII.

IV. *An*

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XLIX. *An*

PHILO-

PHILOSOPHICAL TRANSACTIONS.

Received in 1765.

I. *A monstrous human Fœtus, having neither Head, Heart, Lungs, Stomach, Spleen, Pancreas, Liver, nor Kidnies. By* Claude, Nicholas Le Cat, *M. D. Professor and Demonstrator Royal in Anatomy and Surgery; perpetual Secretary to the Academy of Sciences at* Rouen, *F. R. S. &c. Translated from the* French, *by* Michael Underwood, *Surgeon to the British Lying-in Hospital, in* London.

Read December 1766, and January 1767.

M. F****, a chair-woman, in the parish of *Carville de Dernetal*, aged thirty-four years, was brought to-bed, at nine months end, on tuesday the third of january, 1764, of two children, having already had six.

 This

This laſt labour, in which ſhe was attended by the widow *Mauger*, a midwife of the ſame town, began with ſo conſiderable a diſcharge of water, that it was judged, not without reaſon, that her pregnancy was attended with a dropſy of the *uterus*.

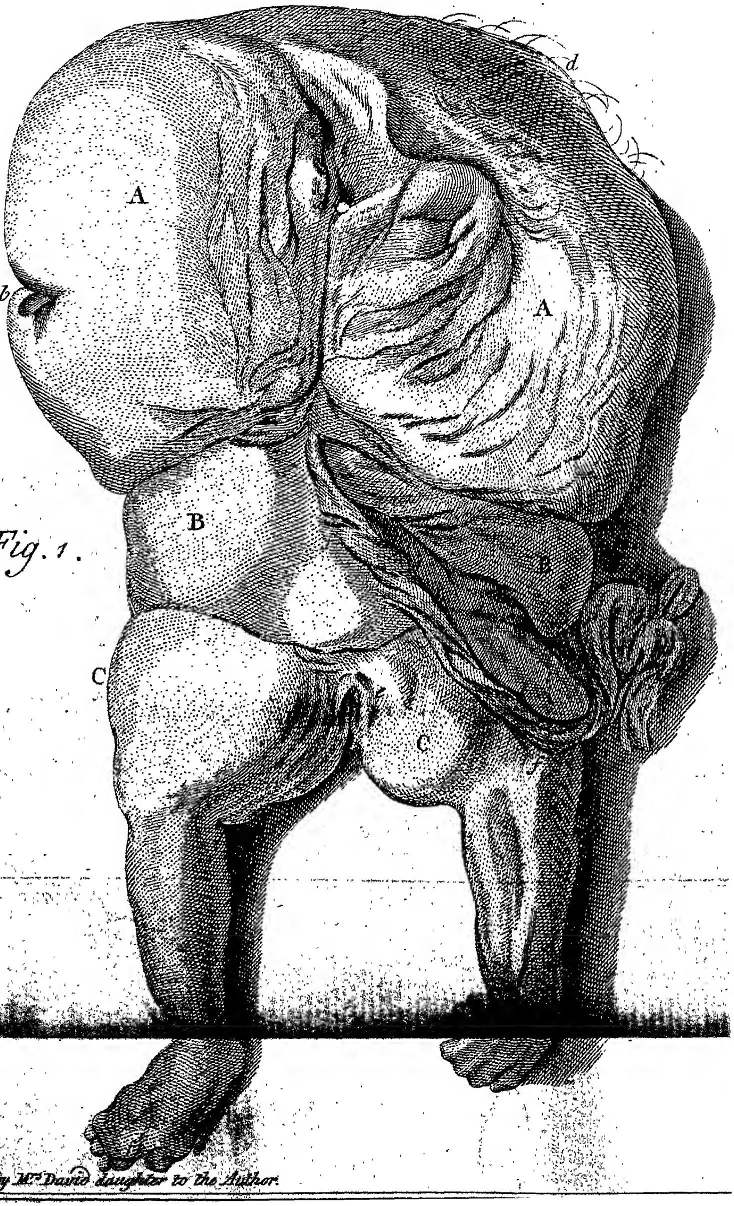
The firſt child was a girl, well formed, who died in the birth, ſolely from the obſtacles which were occaſioned, during the courſe of the labour, by the ſecond child, or monſter, which I am going to deſcribe.

All the lower part of this child, from a finger's breadth above the navel, was likewiſe a female, tolerably well formed, except that on her left foot ſhe had but four toes, joined together by a membrane, like the web of a duck's foot.

But all the parts of this fœtus, above the navel, compoſed a perfect *mola*, a ſhapeleſs maſs, repreſented in the two following figures, of which it will be neceſſary to read the explanation, in order to have a juſt idea of the external appearance of this monſter.

The drawings are reduced to almoſt half the natural ſize, both as to length and breadth; from whence it may be obſerved, that each of the children, who had lived to their full time, had acquired, in this dropſical womb, a bulk pretty near equal to that of other children, born at full time, when twins, and not very thriving. The full ſize of our monſter was twelve inches ſix lines, and the navel was in the middle of this ſpace.

Fig. 1.



Drawn by Mrs Davis daughter to the Author.

Fig. 2.

J. Mynde sc.

TAB. I.

FIGURE 1.

Reprefents the monftrous fœtus in a front view.

A. The upper and only monftrous part, which feems to contain its head, its fuperior extremities, and the breaft.
B. The belly.
C. The lower extremities.

a. Solid and bony eminences, which appeared to be portions of a jaw, without any opening for a mouth.
b. The extremity of the thumb of the right hand.
d. The hair, the fame as on other children.
All the furface of this bag was like the fkin of any other fœtus.
e. The genital-parts of a female.
f. The *umbilical* cord.

FIGURE 2.

Reprefents a back view of the fœtus.

A. The fame bag as at letter A. fig. 1.
B. The loins.
C. The lower extremities.

a. The hair as feen in the former Fig. under which might be felt the bones of the *cranium*.

b. A tranſparent veſicle, like an *hydatide*, which I took for an imperfect eye.

c. Part of the bag, where we obſerved the extremity of a thumb ſeen at *b*, fig. 1.

d. Part of the ſame bag, which reſembled that marked *c*.

e. Under this region might be felt the *ſpina dorſi* and *colli*.

Deſcription of the internal parts of this monſter.

I began the diſſection on the hind part; the muſcles of the back were well formed. I found on the right ſide eight ribs, reckoning from below upwards, and ſeven on the left. Immediately above theſe was an *hydatide*, in which lay the cervical nerves, deſtined for the upper extremities. In this place alſo might be ſeen very imperfect rudiments of the *ſcapula* and *clavicle*.

On the right ſide, and near to thoſe rudiments, was a ſort of thumb, eaſily known to be the thumb of the right hand, whoſe extremity projected beyond the integuments three-fourths of its natural length.

Above, and on the right ſide of this *hydatide*, I diſcovered another more conſiderable, ſurrounded by a large bag, very ſmooth on the inſide, and ſupported by ſomething, which had the appearance of a beginning of *maxillæ*.

At the extremity of the fore part of this bag were two orifices, almoſt contiguous, acroſs a *ſeptum*, which led to another bag of a much ſmaller ſize.

These two *hydatide* bags were behind that represented in Fig. 2. (letter *b.*) which I took for an imperfect eye, because it was transparent, and surrounded by teguments not unlike eyelids.

On tracing the *spine*, and divesting it of all the soft parts, we discovered that it terminated above in a bony mass, that resembled the *larynx*, above which was a large soft substance of the consistence of, and covered with, that kind of skin common to a cow's udder; on which we saw hair like that of other children. This occupied the usual place for the head.

Under this kind of *parenchymatous* substance, which was white and glandular, was a muscular mass, more considerable and conspicuous than one could well have expected in such a subject. It doubtless consisted of the *occipital* and perhaps *frontal* muscles, drawn towards each other.

Having raised this fleshy part, I opened the upper bag of the *spine*, resembling a *larynx*. Its surface was altogether bony, as usual in the fœtus, viz. somewhat cartilaginous. This I opened in the direction of a membranous triangular line, somewhat like the *lambdoidal suture* of the *occiput*. We found this kind of *larynx* fitted with *cerebrum*, or rather *cerebellum*; it might be about a cubical inch in proportion; and this was all the brain of the fœtus.

At the extremity of this cavity, backwards, lay the proper *medulla spinalis*. This cavity was not separated by an elongation of the *cerebellum*, it had but a very small *falx* forwards; and on the right side was another appearance of an elongation of the

cerebellum;

cerebellum, ſo that this ſmall portion of brain did moſt likely belong to the *cerebellum*.

At the baſis of this kind of unformed *cranium*, forwards, was an opening leading to a ſmall brown *hydatide*, ſituated on the right ſide, under a bone which had the appearance of a portion of the *maxilla*, which led towards a ſort of mouth, ſcarce formed, and cloſed; it is repreſented at the letter (*a*) of the firſt figure. There was nothing on the other ſide, no appearance of a mouth, nor any thing that ſeemed the leaſt like it.

I took this *hydatide* for an unformed jugular bag, or true *cæcum*; in the adjoining bone I found a kind of right ear.

The fore part of this ſame ſuperior ſurface of the *cranium* was flat, but a little hollowed, like the upper ſurface of the *larynx*; in the middle was a conſiderable ridge, and on its anterior part appeared a prominence: thus this bone, which ſhould have been ſimilar to the two *parietals*, did not reſemble them at all.

Underneath this prominence, the bone took a perpendicular turn, making a ſharp angle with the upper ſurface, and forming a cavity in its deſcent, which terminated in a projection forwards; it was on the right ſide of this projection where the ſuppoſed right branch of the *maxilla* was attached; within that branch appeared the trace of the jugular above-mentioned, and very diſtinctly the nerve of the eighth pair.

In the breaſt, or rather under the ribs, were neither heart nor lungs, but the ſame white, *parenchymatous* and *œdematous-like* ſubſtance, which we

ſaw in the place of the head. Below this was no *diaphragm*, at leaſt no diſtinct one. In the belly, which extended itſelf juſt under the ribs, was a bundle of inteſtines, and a little red maſs, which I called the liver, for want of a better name, becauſe it ſeemed, that, when I pulled the umbilical cord, this ſubſtance moved, which induced me to believe that the umbilical vein entered there.

No ſtomach, ſpleen, pancreas, or kidneys were ſeen.

The inteſtinal maſs was divided into two portions. The firſt was of a reddiſh colour, which terminated upwards in a blind pouch, and below joined the other portion, as the *ileum* does where it unites with the *colon* and *cæcum*. This ſecond portion was white, and ſeemed to include the large inteſtines. The *cæcum* was very long, or rather the *cæcum* and its *appendix vermi-formis* were of the ſame ſize.

Thus, there was neither *jejunum*, or *duodenum*, or ſtomach, or any liver properly ſpeaking: for that, which I found in the place of it, was a red *viſcus*, and of the *conglomerate* kind, like the kidney in a fœtus. Having cleared it from all its adheſions, I diſcovered neither veſſels analogous to thoſe of the *ſinus* of the *vena portæ*, or any thing that reſembled the figure of the liver, or of any of its appurtenances. I opened it, and was more and more convinced that it was rather a kidney, or knot of renal glands, than a liver, although it was one maſs, and placed in the midſt of the inteſtines; it had ſtill leſs the reſemblance of a heart, having no cavity, no veſſels, or any muſcular fibres.

The

The extremity of the *colon*, or the *rectum*, paſſed betwixt the bladder and the *uterus* as uſual. I diſſected all theſe parts, and traced the bladder up to the umbilical cord, where it lengthened into a pipe, and formed an open *urachus*. It had not the pyriform ſhape of the common bladder.

It was in making theſe diſſections of the *kidney-liver*, and thoſe of the *pelvis*, that I divided the principal veſſels, which I ſhall mention in the ſecond examination.

All theſe inteſtines, and eſpecially the *rectum*, contained excrements of a light aſh colour, but no *meconium*.

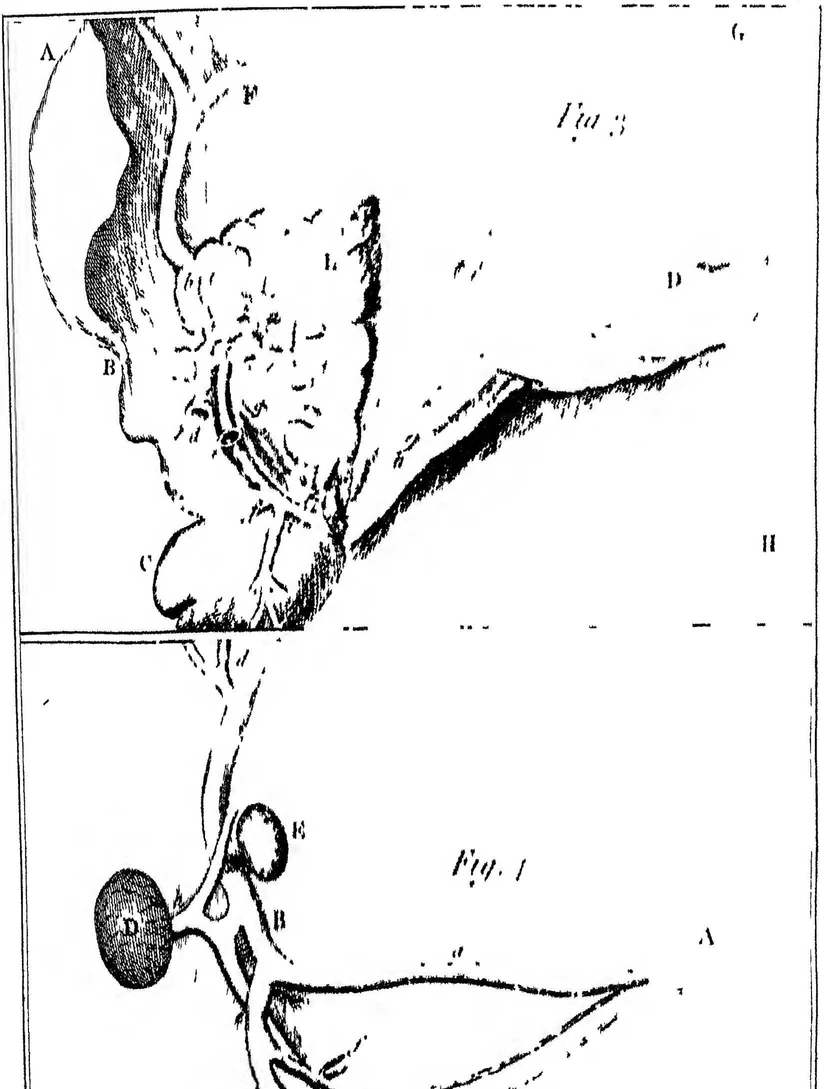
The bladder, although lengthened and continued by an *urachus*, as high as the navel, opened in the uſual place betwixt the *nymphæ*.

The *anus* was imperforate, and the *rectum*, immediately under the *uterus*, terminated in a blind pouch, attached to ſome membranes that went to the *anus*. This pouch was quite full of that ſort of excrement juſt now deſcribed.

Second examination, made the next morning.

I had confined myſelf, in the firſt examination, to the ſingularity of a want of head, lungs, and heart, and of the exiſtence of nerves, notwithſtanding theſe defects. In this ſecond review, the organs of the circulation in ſuch a production, which had lived nine months, raiſed my curioſity; but it was rather too late for my intire ſatisfaction, for I had not taken care enough to preſerve the internal parts, imagining before-hand that they reſembled thoſe

of


A
F
G
Fig. 3
L
D
B
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H
E
Fig. 1
D
B
A

of two headleſs twins, in my poſſeſſion, whoſe inſide was intirely ſimilar to that of other fœtuſes. I diſcovered, notwithſtanding, the following things, and I have repreſented the moſt eſſential in a rough ſketch ſeen in figure 3.

FIGURE 3.

A. Is the region of the breaſt, whoſe internal ſurface was lined with a membrane, which I took to be a *diaphragm* thruſt back and ſtuck to the *pleura*, becauſe it aroſe from lumbar muſcular portions reſembling the pillars of the *diaphragm*.

Under this ſpecies of midriff was a very regular diſtribution of arteries and nerves, which I neglected to delineate, as thinking it uſeleſs; I contented myſelf with diſſecting the *aorta* F. of which the upper ramifications were as uſual.

B. is the lumbar region.
C. Thoſe of the *oſſa ilia*, and of the *pelvis*.
D. The *umbilical* cord paſſing through an opening acroſs the teguments of the lower belly, to be brought into view.
E. The inteſtines.
F. The *aorta ſuperior* before-mentioned.
G. H. The integuments of all the right ſide, opened on the back, to preſerve it whole, and alſo thoſe of the belly; they are thruſt to the right ſide to give a view within.

a. A kind of ſingle kidney, which occupied the place of the liver. The principal part is covered by the inteſtines. It received ſeveral ſmall veſſels from the *aorta*, but none of them was near ſo large as the *emulgent*.

b. A ſmall lobe, that might paſs either for a ſmall lobe of the liver, or for a *capſula renalis*.

d. An orifice of a vein, which was alſo ramified, through the *kidney-liver*, by branches as fine as thoſe it received from the *aorta*. But this vein did not terminate there.

1. It ſent, upwards, branches to the muſcles, to the *vertebræ*, and thence to the ribs; for, by blowing into the trunk, the air came out at the origin of the uppermoſt rib I had diſſected.

2. Below, it formed two large *iliac* veins, *g*. which took the uſual courſe.

e. The trunk cut, belonging to the *aorta inferior*, of the length of a line.

f. The diſtribution of the reſt of this *aorta*, or right *iliac*, through that ſide of the *pelvis*.

g. The *iliac* veins going off to right and left, as uſual.

h. The orifice of the *umbilical* vein, being ſo exactly of the ſame diameter, of the ſame white colour and ſtrength, and of the ſame conſiſtence with the divided trunk, *e*. that at firſt ſight, I took this for the ſame veſſel. The trunk, *d*. was much thinner, and of a more bluiſh tint.

Under

Under the *umbilical* vein, *b.* was the *umbilical* artery on each ſide, which went as uſual into the *pelvis*, and there ſent off the ordinary ramifications.

On the right ſide, its *anaſtomoſis* with the *iliac* artery was very diſcernible; but on the left we could not diſcover the *iliac*, a veſſel which is pretty conſiderable, even in the fœtus, and was ſo on the other ſide, as ſeen at letter I.

On the left ſide, the *umbilical* artery, at its origin, or its inflection, had a kind of web of arteries ſubdivided, ſome of which, doubtleſs, communicated with the *aorta*, or right *iliac*; but neither of theſe branches appeared to be near the ſize of the right *iliac*, nor could we find any thing any-wiſe like to the trunk, *c.* which I long ſuſpected to be the trunk of the right *iliac.*

What veſſels then are theſe trunks, *d. e.*? Which of the two is the continuation of the *umbilical*, *b.*? This is a very important point, but not eaſily determined. It would not have proved ſo, if I had injected the *umbilical* vein, as I generally do in all my monſters; or if I had taken more care of the parts about the *kidney-liver*, which I certainly ſhould have done, had I but ſuſpected ſo many ſingularities.

I ſaid, that at firſt ſight, I took the trunk, *c.* to belong to the *umbilical* vein, and it is ſtill an opinion, to which I am inclined, for the following reaſons.

1. At this part, *c.* the *aorta* had its greateſt circumference; and, in tracing it from this trunk, above as well as below, it grew leſs and leſs. This was, therefore, its trunk, or origin, and could not be a branch of it.

2. I have already ſaid, that the mouths, *b.* of the *umbilical* vein, and, *e.* of the *aorta inferior*, were exactly of the ſame diameter, of the ſame white color, and of the ſame ſtrength; and that the other veſſel, *d.* was much weaker, of a laxer texture, and of a ſomewhat livid color, like the coats of the veins.

3. The *umbilical* vein is, with reſpect to the mother, or to the *placenta*, which tranſmits the blood to the fœtus, a real artery, going from the center to the circumference, or from the principal body, which is the mother, to an adjoining organ, which is the child; and the *umbilical* arteries are properly veins, which return the blood from that adjoining body to the common center of the grand circulation. The blood from the *umbilical* vein then is truly an arterial blood to the fœtus.. In the uſual ſtructure of the *embryo*, nature has ſhortened all the ways, to bring the arterial blood of the mother more ſpeedily into the heart, into the very *aorta inferior* of her fœtus. Therefore, in a ſubject, where there is no heart, or even liver, that vein ought to communicate immediately with the *aorta inferior*. In this manner one conceives how this ſubject could do without a heart, the *umbilical* blood being a continuation of that from the arteries of the *placenta*, the *uterus*, and in ſhort of the mother; the impulſion of the maternal blood was propagated by that *aorta* through all its ramifications both above and below. In one word, the heart of the mother ſupplied that of the fœtus, and the circulation in this was a continuation of that of the mother. Theſe are the reaſons inclining me towards that firſt opinion; and here are thoſe that ſuſpended my judgment for ſome time in favor of the ſuppoſition

of a

of a communication of the *umbilical* vein with the trunk, *d.* of the *vena cava* of the fœtus.

1. In every fœtus the *umbilical* vein empties itſelf into the *vena-cava* in the liver; therefore nature hath here followed her uſual courſe.

2. With regard to the arterial trunk, *e.* it is cloſe by the diviſion, *g.* of the *Iliac* vein, whence it is very likely that it was the left *iliac* vein which was divided.

This laſt, and above all ſtriking reaſon, made me employ almoſt a whole morning, in looking over and over this left *iliac* region, to diſcover the divided veſſel, which would have put the whole matter out of diſpute; but I could find no trace or appearance of it. All the veſſels communicating with the left *umbilical* vein appeared very intire, though deprived of part of their ramifications by which the air eſcaped, but all grew in their courſe leſs and leſs in diameter. Which then was the origin of this left *umbilical* artery? doutleſs the branches of the trunk, *f.* of the *aorta*, which were numberleſs in the *pelvis*, but had partly been ſpoiled the preceeding night, in diſſecting the *rectum*, *uterus*, and bladder of this monſter. I add, that this trunk, *e.* was joined to ſome membranes, which we were obliged to pull about, in order to make it turn to the left, and this direction appeared not to be its natural poſition.

Be it granted, for a moment, that, *e.* is the left *iliac*, and that the *umbilical* vein joins it at, *d.*; how could the blood circulate in this fœtus? How could it have lived the nine months? *d.* is evidently a trunk of the *cava*, which generally enters the right auricle of the heart, dividing, like this, into the

cava

cava superior, *d. b.* which riſes by the *vertebræ* up to F. and into the *cava inferior*, *d g*. 1. It would be abſurd to place the only moving power of circulation in the *vena-cava*, or indeed in any vein. 2. When you have placed it there, what will this ſuppoſition tend to? 3. This vein ſubdivides, and ramifies itſelf through the *kidney-liver*, the muſcles and the ſpine; but none of its branches communicates with the *aorta*. The *aorta* on its part ſends ſeveral branches into the *kidney liver*, very ſlender, and reſembling, by their tranſverſal direction, the common emulgents, but very different in ſize. If then the circulating force were placed at *d*. it could only produce an inverſe circulation, by the communication the *cava* might have by its capillary branches, with the like ramifications of the *aorta*, which ſuppoſition ſeems too much againſt nature to counterbalance the other opinion, which makes the trunk *e*. of the *aorta*, a portion of the *umbilical* vein, and the ſubſtitute of the heart.

Another anatomical fact proves this laſt opinion; which is, that the *aorta*, and eſpecially the ſuperior, F. ran up as high as the *cranium*, and was of a pretty conſiderable ſize, while the venal trunk, *d*. had nothing but capillary branches in the upper parts; ſo that it almoſt appeared evident that the veſſel, *e*. F. was connected with the chief mover of the fluids. Wherefore, ſuppoſing the trunk, *d*. to be the *vena porta*, or an imperfect *cava* going to ſome of the *viſcera*, being the rudiments of an imperfect heart, or a *vena-cava* ending in a pouch, as the inteſtines did which ſhould have entered the ſtomach, if there had been one; the difficulty almoſt diſappears. I ſay almoſt, becauſe, even on this ſuppoſition, if there was a circula-

tion

tion in this monſter; we muſt admit ſome *anaſtomoſes*, between the arterial and venal ſyſtem, which ſupplied thoſe found in other fœtuſes; ſince the venal blood muſt in ſome place or other re-enter the arterial torrent. Such might be the *anaſtomoſis*, K. fig. 4.

For, by this hypotheſis, the vaſcular ſyſtem of this ſubject would be repreſented by fig. 4. in the following manner.

A. The *umbilical* cord.
B. The inteſtinal tube.
D. The kidney-liver.
E. A ſort of *glandula renalis*.

a. The *umbilical* vein; the great mover of the fluids.
b. The *aorta*, a continuation of that vein.
c. d. aorta ſuperior, accompanied by the *vena-cava*.
e. Aorta inferior.
f. The diſtribution of the *iliac*.
g. The *umbilical* arteries, making a part of the diſtribution.
h. The trunk of the *venæ-cavæ* coming either from the *portæ*, or from the *viſcus* D, or forming a blind pouch in that part. Some traces of the *vena-cava ſuperior* appear towards C.

I. The *cava inferior* going to form the *iliacs*.
K. A neceſſary *anaſtomoſis* between the two kinds of veſſels, arteries, and veins. I place it in this conſpicuous ſituation, though it might have been any where elſe.

I repeat

I repeat it again, if I had but ſuſpected ſo many ſingularities, what I now can give only by way of conjecture, might have become demonſtrable in fact. It is ſcarce probable that I ſhall ever have ſuch another opportunity; but it is more ſo, that it may offer to ſome one among the great number of the literati in Europe, who read the Philoſophical Tranſactions. This was the principal motive that determined me to preſent this obſervation, though imperfect, to the Royal Society. Why ſhould we heſitate to make a publick acknowledgement of our faults, when our brethren may profit by, and amend, them?

Another motive, which engaged me to offer this obſervation, ſuch as it is, was, that even the imperfection of it does not affect the uſeful conſequences deducible from it: for, whatſoever may have been the diſpoſition of the blood-veſſels of this monſter, it is a fact abſolutely certain, that it had no heart, nor any other *viſcus* in the place of it; and that the circulation of the fluids, which appears to have taken place from the exiſtence of the principal arteries and veins, could not have had any other moving power than the circulation of the mother itſelf. Hence this child, monſtrous as it is, demonſtrates the circulation of the blood from the mother to the fœtus, and from the fœtus to the mother again; which ſome moderns deny, and others endeavour, at leaſt, to render doubtful. I preſented to the Academy at Rouen ſome years ago, ſeveral obſervations which favoured the antient ſyſtem; the preſent comes to their ſupport, to give this excellent hypotheſis of Harvey all the credit it deſerves.

The

The child I ſpeak of had no mouth, *œſophagus*, nor ſtomach, thus it could not, by that uſual paſſage, be nouriſhed from the waters that ſurrounded it; it could not abſorb from the ſurrounding fluid wherewith to fill its veſſels, and ſupply its growth. It, therefore, follows that it received both its arterial and alimentary fluids from the mother by the *umbilical* cord, and that it owed every thing to that circulation, which ſome would attempt to annihilate.

In the inteſtines of new-born children we find a black excrement, called *meconium*; this black pulp can receive its color only from a bile thickened by retention, and poured directly from the *ductus choledochus* into the *duodenum*. Now this fœtus, having no liver, nor gall-bladder, &c. could have no *meconium*; therefore the pulp found in the inteſtines was of an aſh-colour.

This monſter had ſo little brain, that that *viſcus* muſt have been of ſmall import towards the functions of this animal. And yet all theſe brainleſs fœtuſes are very lively. Mr. Denis, who, in his twelfth conference, has given an account of one of them, and M. Vaiſſiere, who ſent me one from Toulouze, the laſt year, both remark, "that theſe children "are remarkably lively in the mother's belly; "that they were in violent motion, at the time "of labor; that the moment they were in the "world they ſeemed ſuffocated, and became all at "once motionleſs." This is a matter worthy of much reflection.

How can we conceive there can be ſenſation and motion, without almoſt any brain in one of theſe monſters; and abſolutely without any in the other?

Senſibility, ſenſations, and paſſions, may exiſt without the brain, and have their ſeat in its *meninges*, and in the coats of the nerves formed by thoſe *meninges*. If I had not endeavoured to prove that point in my phyſiology, and lately in my diſſertation on the ſenſibility of the *dura* and *pia mater*, the obſervations of children and animals born without brain, which are pretty conſiderable in number, would demonſtrate it by facts.

That theſe children are more lively, that is, more ſenſible, I attribute to their having little of the nervous juice, though not leſs of the active fluid. This nervous juice I termed *fluide conſervateur*, the preſerving fluid, in my phyſiology. It is long ſince I have obſerved that an abundance of this nervous juice produces the contrary of vivacity; and, as a conſequence, of our principles, long ſleep, or rigid continency for a length of time, renders us heavy and benummed, becauſe in each of theſe circumſtances, this nervous juice abounds and regurgitates, if I may ſo ſpeak.

But theſe violent motions, whence have they their origin? there muſt be a nervous juice, to act in the muſcles, and here we have very little.

Neither of theſe animals was deprived of the *medulla ſpinalis*, and one of them had a ſmall portion of brain, or *cerebellum*. This is one ſource of the nervous juice, and of the active fluid, neceſſary to muſcular motion. This ſource, I grant, is weak and poor, but I have made it appear in my treatiſe on this ſubject, which obtained the prize from the Academy of Berlin, that there is in the blood a richer ſtore, which the nervous fluid unites to, and

makes uſe of, in muſcular motion. We explain by this, how it happens that an aſs, who has ſo much leſs brains than a man, is, notwithſtanding, ſo much ſtronger, becauſe it has much more blood.

Here then is a ſecond ſpring that affords theſe monſters a conſiderable ſupply; but though ſufficient for their motions, it is not equal to that of an ordinary fœtus, and the violent agitations of their body ariſe from their great ſenſibility, which we have juſt now accounted for. Now, the blood in the fœtus, and eſpecially in theſe, belongs to the mother; they are furniſhed by her, as well with air, as with the nervous juice, and the animal fluid, which are eſſential to her. Wherefore, as ſoon a theſe children are ſeparated from the mother, and deprived of that vital ſource, all motion muſt ceaſe in them, as if they were ſuffocated, that is, as in any other ſuffocated fœtus.

Let us conclude this account by a word or two on the cauſe of theſe monſters.

The great quantity of waters voided by the mothers of theſe children, proves that the principle of their monſtrocity is a diſeaſe, a ſort of dropſy, and even a kind of *hydrocephalus*, which had run off a conſiderable time before the labor.

The two *hydatides* I found at the origin of the *brachial* nerves, and which had evidently been the cauſe of the mutilation of the upper extremities, are examples that help us to comprehend that of the other organs. On ſuppoſing a like diſorder on the origin of other nerves, which have their riſe from the brain, it will be obvious that the organs, to which theſe nerves run, that is, where they convey the nervous

fluid, which contain the rudiments of every part, will be wanting. It may indeed be ſaid, there are *hydrocephali* that have all the organs very well formed: but there the diſeaſe has commenced after the perfect formation of theſe parts, whereas if you ſuppoſe it to have happened in the very time of that formation, you will ſee that the nervous juice, vitiated, diluted, and turned out of its natural courſe, can no longer be employed in the generation of thoſe organs.

Thoſe who have attended my courſes, and have read my phyſiology, will be pleaſed to ſee that all theſe myſteries, which one would have thought impenetrable, are eaſily accounted for upon the principles I have laid down.

Received October 26, 1766.

II. *A Letter to Dr.* Watſon, *F. R. S. containing a Deſcription of Three Subſtances mentioned by the* Arabian *Phyſicians, in a Paper ſent from* Aleppo, *and tranſlated from the* Arabic, *by Mr.* J. Channing, *Apothecary.*

S I R,

Read Jan. 8, 1767. AT your deſire, I ſend you the tranſlation of the Arabic, and the ſpecimens which you ſaw at my houſe. The paſſages included within hooks, with an aſteriſm before them, are added, to make it more intelligible. I ſend likewiſe a copy of the paper which came with the ſpecimens, written by a gentleman of the factory at Aleppo. It will give me pleaſure, if they ſhould be thought worthy of the inſpection of the Society.

You will eaſily gueſs at my view in procuring theſe ſpecimens. The Tabaſhir, Mamithſa, and Mamiraan are uſed by the Arabian phyſicians; by Rhazes particularly; in page 62 (not. 32) page 110 (not. 4); page 146 (not. 6). I have given the beſt account of each, which I could meet with; and you will ſee it differs not much from this paper, which came after that book was finiſhed and printed off.

Laſt.

Laſt week, in the Bodleian Library, I met with an Arabic MS. of Dioſcorides. It appears quite intire and perfect. The Greek titles are inſerted in the margin by Dr. Hyde, in red ink, which is ſome proof of his value for this MS. To me it appears a real treaſure, and it is likely it may be of excellent uſe in correcting the very corrupt text of that author: perhaps too it may be a means of aſcertaining the Materia Medica of the elder Greek phyſicians. The Eſcurial MS. contains only the firſt three books, and is imperfect at the beginning. If a tranſcript of this latter, however, could be obtained, it might likewiſe be of great uſe.

I am,

SIR,

Your moſt obedient

humble ſervant,

Eſſex-ſtreet,
Oct. 24. 1766.

John Channing.

Copy

Copy of a Paper ſent with the Specimens, by a Gentleman of the Factory at Aleppo.

THE ſpecimens ſent of the Tabaſheer, though taken from different parcels, are not regarded here as different ſorts. Amongſt them will be found one or two pieces, which in their form anſwer to the ancient Arabic deſcription of this drug. It is not from the ſugar cane that the Tabaſheer is ſuppoſed to be procured, but from that kind of cane of which the Arabs make their lances, and of which a piece is herewith ſent.

Different opinions concerning this ſubſtance, as alſo concerning the Mamithſa and Mameraan, collected from ſundry Arabian writers, will be found in the paper incloſed: but as the Tabaſheer is brought from the Eaſt, not prepared here, I cannot affirm any thing certain about it.

From the ſpecimens ſent of the Mameraan, it will appear evidently to be a root. It is commonly believed to be a ſpecies of the Chelidonium, and, like the Tabaſheer, brought from the Eaſt to Aleppo.

Mamithſa, ماميشا or ماميثا, is the common name uſed here for wormwood. Our mint is called Nana نعناع. The literary name, however, of wormwood is افسنتين (* Iſsantin, abſinthium).

But there is a plant here known by the name of ماميثا (* Mamitha) of which a ſpecimen is now ſent. This, from the uſe made of it by the natives in diſtempers of the eyes, as well as from other circumſtances, appears to be the Glaucium of Dioſcorides.

Dioſcorides. An Papaver corniculatum floribus cæruleis?

Concerning the Iſbidrowia, I have been able to get no intelligence. The Orichalcum is called here طارج or جام (* Dgaam or Tûck).

The paper made of ſilk huſks is not to be found at preſent in the city. If any can be procured from the Baſſora caravan lately arrived, it ſhall be ſent.

Tranſlation of a Paper in Arabic, *ſent to* W——— C———, *Eſquire, from* Aleppo, *with ſeveral Specimens of* Tebaſhir.

طباشير, with a طاء. Tebaſhir.

In the Camus (* an Arabic Lexicon, which the celebrated Golius tranſlated into Latin), Tebaſhir is ſaid to be a ſubſtance found in the hollow of that ſpecies of Indian cane of which lances are made: or the lower part of that cane burnt. The Tebaſhir which is formed at the knots of the cane is round like a dirhem (* ducat). This ſubſtance is found in the cavity of thoſe canes which have been fired by rubbing one againſt another. It is frequently adulterated: the burnt bones of ſheep, the ſkulls particularly, are ſold for it.

Ebn Beitar, in his treatiſe of ſimples, ſays, "Tebaſhir is a ſubſtance found in the hollow of the Indian cane."

Ali Ebn Mohammed ſays, "It is the burnt part of the lower ſtem of the Indian cane; and is imported only from the coaſt of India; chiefly from that part of the coaſt called Sendapour

" (* or

" (* or Sendaſour) from whence the black pepper
" is brought."

Avicenna, in his Canon, ſays of Tebaſhir, " it is " the lower part of the cane which has been burnt; " it is reported that the canes are fired by being " rubbed one againſt another by the violence of the " winds. This drug is produced on the coaſt of " India."

الماميثا Mamitha.

Ebn Beitar in his Treatiſe of Simples calls it Mamitha. Abu'l Abbas the Nabathæan (* the botaniſt) calls it Mamithſa. (* i. e. the letter is written ſometimes with ت two, ſometimes with ث three points over it). Both theſe names are ſufficiently known.

In the treatiſe of ſimples called Ma-la-Yeſa (* i. e. a treatiſe of thoſe things which no phyſician ought to be ignorant of) ماميثا Mamithſa, is " the name " of a plant like the papaver maritimum, or cornicu- " latum. At the lower part of the Mamithſa is a " moiſture which ſticks to the hand: it has a yel- " low flower like the papaver before mentioned; its " ſeeds are different, inclining to black, like and " about the ſize of the ſeeds of ſeſamum. The " plant is of a ſtrong and offenſive ſmell, and very " ſucculent. The difference between theſe two " plants is this; the papaver corniculatum dies to the " root in the winter, and ſprouts again from its root " in the ſpring; the Mamithſa, on the contrary, " ſprouts again in the ſpring from the top of its ſtem."

Avicenna, in his Canon, ſays, " Mamithſa is " like acorns, of a yellow colour inclining to black, " eaſily

" eaſily broke. It is bitter, of a ſubſtance watery " and earthy; cold, but not vehemently ſo; its " juice is in the ſame degree of cold as the wa- " ter of pools or lakes. It is prepared from a plant " which is brought from Manbedge" (* a town of note in Syria, vid. Geogr. Nubienſ. page 120, line 7, and Index Geograph. in vitam Saladini, in voc. Man-besjum) " of a very diffuſive ſcent, a bitter taſte, " whoſe juice is yellow, of a ſaffron colour."

Mamiraan. In the Liber Memorialis, it is ſaid, " Mamiraan is a plant, at the bottom of whoſe " ſtem are produced knotted, crooked, hard roots. " The Indian is the beſt; this inclines to a black " colour: the Chineſe to yellow: the other ſorts are " green. It grows in the water; its leaf is like " the leaf of the convolvulus; it is hot and bitteriſh; " its ſeed is like that of ſeſamum."

It is ſaid in the Canon of Chalid and Manown, " ſome ſay it is a root, and called Mamiraan; others " ſay, the ſmaller roots are called Mamiraan, but " the larger Zeradguſh" (* in Caſtell's Lexicon, col. 308, and in Meninſki, col. 2441, the word is زردجوب Zeradgiob, which ſignifies yellow wood, and is the Perſic name for curcuma).

Avicen, in his Canon, ſays, " Mamiraan is a " woody knotted ſubſtance, inclining to a black " colour, has ſmall curvatures, and is one of the " things uſed by dyers."

Ma-la-Yeſa ſays, " Abſinthium is a Greek word, " in Perſic it is called Mowi Chowſheh. This is a " plant which grows freely and largely; it riſes in " a ſtem, from which ſhoot out many branches,

" on

" on which are many thick and tufted leaves; it " bears a flower like that of parthenium, ſmall and " white; in its middle it has a part yellow; its head " is ſmall, in which is a ſmall ſeed; its taſte is bit- " ter and ſtyptic. Some ſorts of it have a leaf like " the daucus, and a yellow flower. The inhabitants " of Egypt call this kind of it Demſiſah. It grows " plentifully in the Eaſt, and in Syria, Choraſan, and " Irak. The two laſt ſorts of it are leſs eſteemed, " and of leſs value."

Abſinthium " ſome phyſicians call this Alſhich " Alroumi" (i. e. Abſinthium Ponticum, or Romanum).

Look into the Canon of Avicenna, under the article Abſinthium, you will find there ſeveral things concerning Mamithſa. Conſider that article, therefore, very attentively.

Received January 8, 1767.

III. *A general Investigation of the Nature of the Curve, formed by the Shadow of a prolate Spheroid, upon a Plane standing at right Angles to the Axis of the Shadow; in a Letter to the Royal Society, by Mr.* George Witchell, *F. R. S.*

Gentlemen,

Read Jan. 15, 1767.

I Beg leave to lay before the Royal Society the following investigation of an irregularity in the duration of the eclipses of Jupiter's satellites, occasioned by the figure of his body.

It has been known for a long time, that Jupiter's body was not truly spherical, but a prolate spheroid, and that in a much greater degree than any of the other planets; but notwithstanding this, it was never suspected that it would affect the durations of the eclipses of the satellite, till Dr. Bevis first thought of it, in the latter end of the summer 1761.

The Doctor, being at that time indisposed, recommended the subject to my consideration; and, in consequence of his request, I not long after presented him with a solution of the problem, being in substance

ſubſtance the ſame with this, as far as propoſition V. a copy of which he ſoon after tranſmitted to that excellent mathematician the late M. Clairaut.

In March 1763, M. de la Lande, an eminent French aſtronomer, being here, Dr. Bevis ſhewed him my paper; this occaſioned a new article in the *Conn. des Mouv. Celeſt.* 1765, p. 177, under the title, *Inégalité dans les demi-durées des éclipſes des ſatellites de Jupiter, cauſée par l'applatiſſement de Jupiter:* in which he mentions this circumſtance in the following words; " M. le docteur Bevis me fit voir à Londres, au mois " de Mars dernier, une ſolution rigoureuſe & al- " gebraique de ce probleme, qui conſiſte à trouver la " courbe qui réſulte de la ſection de l'ombre d'un " ſpheroide à une diſtance quelconque."

In this ſtate it remained ever ſince; for though the Doctor, and ſome other gentlemen, to whom I ſhewed it, frequently urged me to lay it before the Royal Society; I always declined it, till I ſhould have time to make ſome farther additions to it.

A few months ſince, M. Bailly, a French gentleman, publiſhed at Paris an elaborate treatiſe upon the theory of Jupiter's [illegible]; in which he has been pleaſed to give the honour of this diſcovery intirely to M. de la Lande, without the leaſt mention of Dr. Bevis. I then thought it incumbent on me to do juſtice to the Doctor, by immediately finiſhing my paper in the beſt manner I was able, and preſenting it to the Royal Society.

I ſhall be extremely glad, if this rude eſſay ſhould excite ſome more able perſon to purſue the ſubject in the manner it deſerves; for though, I believe, my ſolution will not be deficient in point of truth, I am

not

not vain enough to think it may not be performed in a more elegant manner. I have the honour to be,

Gentlemen,

Your moſt obedient

humble ſervant,

Fleet-Street,
Jan. 7, 1767.

George Witchell.

LEMMA.

If any ſpheroid is cut by a plane, in any direction whatever (excepting that which is perpendicular to its axis), the figure of the ſection will be an ellipſis. This is demonſtrated in Simpſon's Fluxions, Vol. II. p. 456.

PROPOSITION I.

Tab. III. fig. 1. Let the ſphere BEGK be cut through its center by the planes BGK, BPD, B*o*D, BOD, EAK, and LPH; it is required to determine the inclination of the planes LPH, BOD, and alſo the inclination of the right lines AC, BC, which is meaſured by the arc AB; there being given the angles of inclination EBF, FB*a*, together with the arc BF: the angles AFB, EAL, being right angles, and the inclination of the required plane BOD, but little exceeding that of the given plane B*o*D.

Let

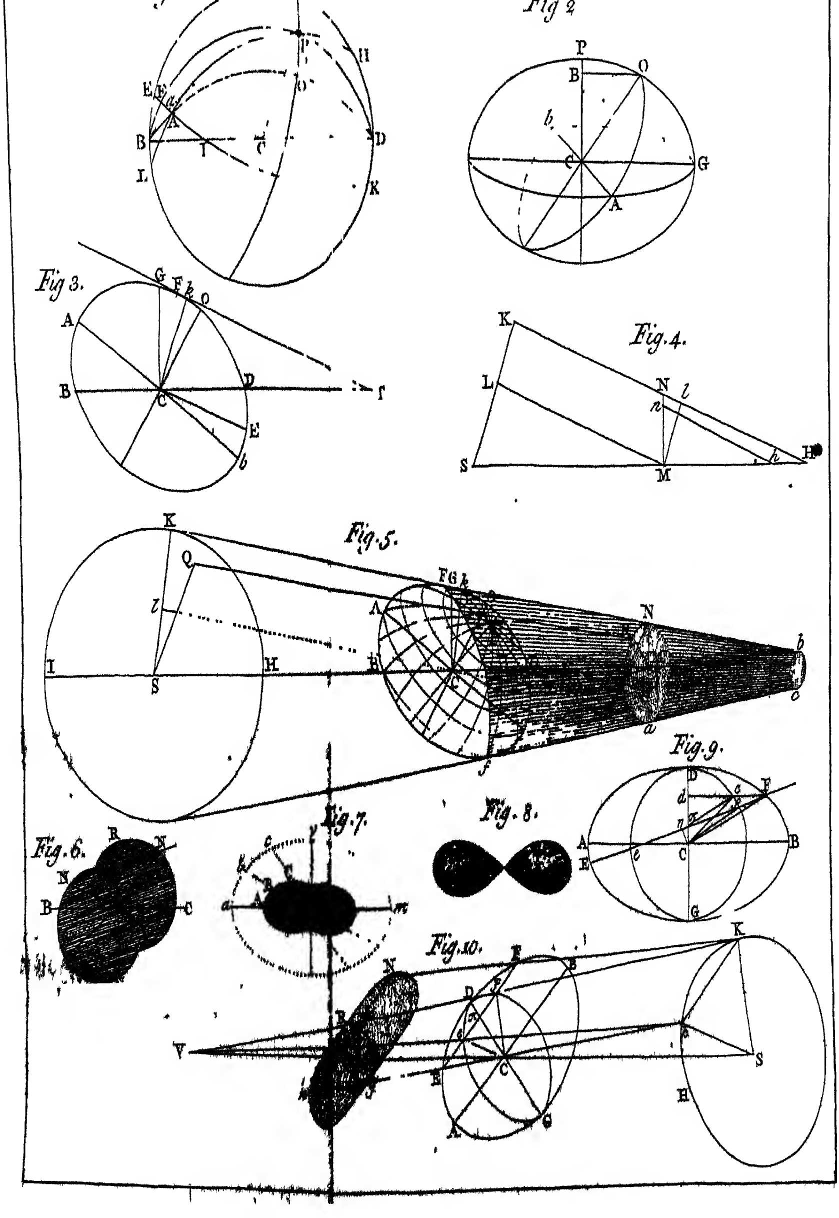
Fig 1
Fig 2
Fig 3.
Fig.4.
Fig.5.
Fig.6.
Fig.7.
Fig. 8.
Fig.9.
Fig.10.

Let the ſine of EBF $=a$, its coſine $=a'$, the tangent of BF $=\beta$, its coſine $=b'$, the ſine of FB$a=p$, its co-ſine $=p'$, the ſine of aBA $=z$, the ſine of AB $=Z$, its coſine $=\acute{Z}$, the ſine of PAO $=\zeta$, and radius $=1$; then we ſhall have the ſine of ABF $=$ the ſine of $\overline{\text{FB}a+a\text{BA}}=p+p'z$, and its coſine $=p'-pz$: Therefore by trigonometry we ſhall have in the right angled ſpherical triangle ABF, as Rad. (1) : coſine BF (b') :: ſine ABF ($p+p'z$: coſine BAF $=$ ſine of LAB, or its equal PAO; therefore $\zeta=b'\times\overline{p+p'z}=$ the ſine of the required inclination of the planes LPH, BOD. In like manner in the ſame triangle it will be as rad. (1) : cotan. BF $(\frac{1}{\beta})$ :: coſine ABF $(p'-pz)$: cotan. BA $=\frac{\acute{Z}}{Z}$; hence $Z=\frac{\beta}{\sqrt{\beta^2+\overline{p'-pz}|^2}}$, and $\acute{Z}=\frac{p'-pz}{\sqrt{\beta^2+\overline{p'-pz}|^2}}$, which are the ſine and coſine of the required arc AB.

COROLLARY I.

If inſtead of a ſphere we now ſuppoſe BEGK repreſents a prolate ſpheroid, whoſe axis is CP; the figures of the ſections LPH, BOD, &c. inſtead of circles, will become ellipſes (by the lemma); but it is evident that the inclinations of thoſe planes to each other, and likewiſe the inclination of the right lines AC, BC, or the angle ACB, will remain unaltered.

COROLLARY II.

If BEGK repreſents any primary planet revolving about the ſun in an orbit whoſe plane co-incides with

with the plane BCD, it is manifeſt that BCD will be its ecliptic, making the angle of obliquity BIE with its equator EAK (whoſe pole is P); and if B be the place of the ſun in this ecliptic, at any given time, the arc BI will be the diſtance of the ſun from the neareſt equinoctial point I; and the arc BF his declination at the ſame time.

COROLLARY III.

If the plane POG, which paſſes through P, the pole of the ſpheroid, be perpendicular to the plane LPH, it will alſo be perpendicular to any other plane BOD, which paſſes through A, the interſection of the equatorial plane EAK, with the plane LPH; therefore the angle ACO being a right angle, it is evident that AC will be the ſemi-tranſverſe, and CO the ſemi-conjugate axis of the elliptic ſection BOD.

COROLLARY IV.

Hence it appears, that the tranſverſe axis of any elliptic ſection BOD, made by a plane paſſing through the center of the ſpheroid, will always be equal to the equatorial diameter of the ſpheroid, but the conjugate axis will be longer or ſhorter, according as the inclination of the planes LPH, BOD, is more or leſs.

PROPOSITION II.

Fig. 2. To find the length of the ſemi-conjugate axis CO, of the elliptic ſection AO*b*, formed by a plane cutting the given prolate ſpheroid POG through its center C, and making the angle PCO with the axis CP.

Let

Let the ſine of the angle PCO $=\zeta$, CG $=t$, CP $=c$, CO $=\kappa$, (radius being unity); draw PO perpendicular to CP: then in the right angled plane triangle BCO, we have as rad. (1) : CO (κ) :: ſine PCO (ζ) : BO ($\kappa\zeta$); and rad. (1) : CO (κ) :: coſine PCO ($\sqrt{1-\zeta^2}$) : BC ($\kappa\sqrt{1-\zeta^2}$); but from the nature of the ellipſis we have $\frac{c^2}{t^2}\times\overline{t^2-\kappa^2\zeta^2}$ $=\overline{BC}^2=\kappa^2-\kappa^2\zeta^2$; therefore $\kappa^2=\frac{t^2c^2}{t^2-\overline{t^2-c^2}\times\zeta^2}$, or putting $t^2-c^2=f^2$, and $t^2-\kappa^2=\varphi^2$, we have $\kappa^2=\frac{t^2c^2}{t^2-f^2\zeta^2}$, and $\varphi^2=\frac{t^2\times\overline{1-\zeta^2}}{\frac{t^2}{f^2}-\zeta^2}$.

PROPOSITION III.

Fig. 3. Let BOD be an ellipſis, whoſe tranſverſe diameter Ab makes the angle ACB, with the right line BCD, and let TkG be a tangent to the ellipſis, in the point F, making the angle GTC with the right line BCD: It is required to find the length of the normal Ck, drawn from the center of the ellipſis, to the tangent TG.

From C, the center of the ellipſis, let CE be drawn parallel to the tangent TG, meeting the ellipſis in the point E; and CG perpendicular to the line BCD, meeting the tangent in the point G: Put the ſine of ACB $=$ Z, its coſine $=\acute{Z}$, the ſine of TGC $=$ V, its coſine $=\acute{V}$ (radius being unity) AC $=t$, CO $=\kappa$, and $t^2-\kappa^2=\varphi^2$; then will the ſine of OCE ($=$ the ſine of OCD $+$ DCE) be

be expressed by $\acute{Z}V + Z\acute{V}$, and by the last proposition we shall find $CE = \frac{tx}{\sqrt{t^2 - \varphi^2 \times \overline{\acute{Z}V + Z\acute{V}}^2}}$; but (by conics) CE × C$k$ = CO × CA, whence we shall obtain $Ck = \sqrt{t^2 - \varphi^2 \times \overline{\acute{Z}V + Z\acute{V}}^2}$.

PROPOSITION IV.

Fig. 4. In the two similar right angled plane triangles HKS, HMN, right angled at K, and M, there is given the right lines KS and MS, to find the acute angles, supposing the given angle hnM to be nearly equal to the required angle HNM. Put MS $= \Delta$, KS $= r$, MN $= v$, the sine of the given angle hn M $= q$, its cosine $= q'$, the sine of HNM $=$ V, its cosine $= \acute{V}$, the sine of $\overline{\text{HNM} - hn\text{M}} = x$, and radius $= 1$. Let ML be drawn parallel to HK, and Ml parallel to SK: then in the right angled plane triangles NMl, SML, we have as rad. (1) : MN (v) : : sine HNM (V) : Ml (vV), and as rad. (1) : MS (Δ) : sin. LMS ($\acute{V}$) : LS ($\Delta\acute{V}$); but Ml + LS = KS; therefore $v\mathrm{V} + \Delta\acute{\mathrm{V}} = r$, and by the foregoing notation $\mathrm{V} = q + q'x$, and $\acute{\mathrm{V}} = q' - qx$; therefore these values of V and $\acute{\mathrm{V}}$ being wrote in the above equation we shall find $x = \frac{q'\Delta + qv - r}{q\Delta - q'v}$, and from thence $\mathrm{V} = \frac{\Delta - q'r}{q\Delta - q'v}$, and $\acute{\mathrm{V}} = \frac{qr - v}{q\Delta - q'v}$.

PROPO-

PROPOSITION V.

Fig. 5. If the opake prolate ſpheroid BPOD, given in ſpecies and poſition, be oppoſed to the given luminous ſphere HKQI at the given diſtance CS, forming the ſhadow $Ffbc$: It is propoſed to determine the figure of the ſection aRN made by a plane, cutting the ſhadow perpendicularly to its axis at the given diſtance MS.

Let the required curve aRN be conceived to be generated by the extremity R, of the variable right line MR, revolving about the given point M as a center, the line MR being always perpendicular to the axis of the ſhadow MS: Let the right line RQ be a tangent to the ſphere HKQI in the point Q, and in the ſame plane with the right lines RM, MS, it will then repreſent one of the rays of light, which conſtitute the conical ſuperficies of the ſhadow, and, therefore, by the laws of optics, will be a tangent to the ſpheroid alſo; now when the generating point R has arrived at N, the ray RQ (being ſuppoſed to revolve with it) will co-incide with the tangent NK, touching the ſphere in K, and the ſpheroid in F: Join K, S, and the angles NMS, and NKS, will be right angles; let the ſpheroid be ſuppoſed to be cut, by the quadrangular plane NMSK, forming thereby the elliptic ſection BOD, draw Ck perpendicular, and Cl parallel to NK; put CA $= t$, MC $= \delta$, CS $= d$, MS $= \Delta$, SK $= r$, MN $= u$, CO $= x$, the ſine of bNM $= V$, its coſine $= \acute{V}$, the ſine of ACB $= Z$, its coſine $= \acute{Z}$, and radius $= 1$:

Then

Then in the right angled plane triangle $C l S$, it will be as rad. (1) : $C S$ (d) :: fine $S C l$ ($\acute{V}$) : $S l$ ($d\acute{V}$), and confequently $C k$ ($= K S - S l$) $= r - d\acute{V}$; but by prop. III. $C k = \sqrt{t^2 - \phi^2 \times \overline{\acute{z}v + z\acute{v}}|^2}$, whence we fhall have $r - d\acute{V} = \sqrt{t^2 - \phi^2 \times \overline{\acute{z}v + z\acute{v}}|^2}$: Now by propofition I. we fhall find $\zeta = b' \times \overline{p + p'z}$, $Z = \frac{\beta}{\sqrt{\beta^2 + \overline{p' - pz}|^2}}$, and $\acute{Z} = \frac{p - pz}{\sqrt{\beta^2 + \overline{p' - pz}|^2}}$; by prop. II. $u^2 = \frac{t^2 c^2}{t^2 - f^2 \zeta^2}$, and $\phi^2 = \frac{t^2 \times \overline{1 - \zeta^2}}{\frac{t^2}{f^2} - \zeta^2}$; laftly by prop. IV. $V = \frac{\Delta - q'r}{q\Delta - q'v}$, and $\acute{V} = \frac{qr - v}{q\Delta - q'v}$, which values being fubftituted in the above equation will exhibit the nature of the required curve aRN, in terms of z and u.

SCHOLIUM.

If the fphere HKQI reprefents the fun, and the fpheroid BPOD one of the primary planets, it will appear, from the preceding reafoning, that the figure of the fection of its fhadow received upon a plane, which is perpendicular to its axis, will not be a circle (except when the axis of the planet produced paffes through the fun's center) but a curve of the oval kind, whofe fpecies will be known from the foregoing equation.

If the fphere HKQI had been regarded as a fpheroid in the above folution, it is eafy to fee that the foregoing procefs would have determined the nature

nature of the required curve; but the figure of the ſun is ſo nearly ſpherical, that it was not thought neceſſary to embarraſs the ſolution with that conſideration.

Hence the duration of an eclipſe of a given ſatelles may be determined in the following manner: Let B R C (fig. 6.) be the ſection of the ſhadow, through which the ſatelles paſſes, NpN the path of the ſatelles, making the given angle NpM, with the circle of latitude RpM; B M C a part of the primary's orbit produced, and Mp the given latitude of the ſatelles at the time of the ſyzygia; the circle of latitude RpM is repreſented in fig. 1. by the primitive circle B E G D, and the angle R M N, by the ſpherical angle E B A; therefore the ſine of R M N = the ſine of E B A = the ſine of $\overline{EBF + FBa + aBA}$ $= ap' + a'p + \overline{a'p' - ap} \times z$, and its coſine $= a'p' - ap - \overline{ap' + a'p} \times z$; which for the ſake of brevity may be expreſſed by y, and y'; then putting $Mp = n$, $MN = v$, the ſine of $MpN = m$, its coſine $= m'$, and radius $= 1$; we ſhall have the ſine of M N P expreſt by $my' + m'y$; and therefore we ſhall have in the plane triangle MpN, as the ſin. MNp $(my' + my) : Mp\ (n) ::$ ſine $MpN\ (m) : MN\ (v)$; hence $v = \frac{m.n}{my' + ym'}$; from which, and the equation of the curve (determined above) $\frac{vy}{m} = pN$, and conſequently, the duration of the eclipſe will become known.

In prop. I. the ſine of the angle ABF is expreſſed by $p + p'z$, and its coſine by $p' - pz$, inſtead of their true values $pz' + p'z$, and $p'z' - pz$; this was done

to

to render the following conclusions more simple than they otherwise would have been; and as the angle aBA is, by hypothesis, but small, its cosine will approach so near to the radius, as not to occasion any sensible error in the result; and the same may be observed with regard to what is advanced in prop. IV.

It remains now to apply, what has been investigated above, to the eclipses of Jupiter's satellites, and to examine whether the prolateness of his figure will have any sensible effect upon their durations; and this is become the more necessary, as that celebrated astronomer M. de la Lande (who candidly acknowledges, that he was excited to turn his thoughts upon this subject, from a cursory view of this paper, which was shewn him by Dr. Bevis*) does not seem to have considered the question, with that degree of attention which I think it demands.

But before this can be done with exactness, it will be necessary to have the inclination of Jupiter's axis, with respect to his ecliptic, and the place of his equinoxes determined by observation, neither of which I believe has yet been done with any degree of certainty; I shall, therefore, proceed in this inquiry upon M. de la Lande's hypothesis, that Jupiter's axis is perpendicular to his orbit; and perhaps this supposition is not so far distant from the truth, as to occasion any material error in the conclusion. It may also be remarked, that in the general equation given above, $\dot{V}$ and V express the sine and cosine of the semi-angle of the cone of Jupiter's shadow, but this angle can never exceed $3'$, and consequently we may very

* Vid. Connoiss. des Mouv. Celest. 1765, p. 177.

safely

ſafely uſe the radius inſtead of V wherever it occurs.

By this means the general equation will become $r - d\acute{V} = \sqrt{t^2 - \varphi^2}$, or which is the ſame $r - d\acute{V} = x$, therefore $\acute{V} = \frac{r-x}{d}$; but by prop. IV. $\acute{V} = \frac{qr - v}{q\Delta - q'v}$, which, becauſe q is nearly equal to V, and $q'v$ very ſmall with reſpect to $q\Delta$, will become $\acute{V} = \frac{r-v}{\Delta}$; therefore $\frac{r-x}{d} = \frac{r-v}{\Delta}$, from which we ſhall find $v = \frac{\Delta x - \delta r}{d}$; and this equation is exactly the ſame with that which would ariſe from conſidering the ſun as a circular, and Jupiter as an elliptic plane, limited by one of his meridians, and always parallel to the diſk of the ſun; which ſuppoſition, the immenſe diſtance of Jupiter from the ſun renders very allowable.

From this equation an eaſy mechanical method may be derived of delineating the curve of the ſhadow, at any given diſtance from Jupiter, for as x denotes any ſemi-diameter of the elliptic ſection of Jupiter's body, it is manifeſt, that the term $\frac{\Delta}{d} \times x$, will expreſs the correſponding ſemi-diameter of a ſimilar ellipſis, whoſe axes are to thoſe of Jupiter in the given ratio of Δ to d, and the term $\frac{\delta r}{d}$ is wholly given: Therefore if arm (fig. 7.) be ſuch an ellipſis, and there be drawn through its center M any number of ſemi-diameters Ma, Mb, Mc, &c. meeting the ellipſis

lipſis in a, b, c, &c. let aA, bB, cC, &c. be taken each equal to the given term $\frac{\delta r}{d}$, and the points A, B, C, &c. will be in the required curve.

It appears from conſidering the nature of this curve, that it will have two cuſps, one at each extremity of its leſſer axis, which will approach toward each other, according as the diſtance δ is augmented; therefore, if the diſtance of the ſection of the ſhadow, from Jupiter's center, was taken, ſuch that $\delta = \frac{dc}{r-c}$, the leſſer axis of the curve would then vaniſh, and the cuſps meet in the center, and thereby form two diſtinct ſhadows (as repreſented in fig. 8); in conſequence of which, if a ſatelles, revolved at that diſtance, it might ſuffer a double eclipſe, at the ſame conjunction, which remarkable phænomenon may alſo happen, at a leſs diſtance from Jupiter, in ſome circumſtances.

I ſhall now ſhew how the duration of an eclipſe of a given ſatelles may be determined independant of the equation of the curve; and this, perhaps, will be the more acceptable, as it will afford a practical rule, which may be applied, in every poſition of Jupiter's axis, with very little trouble. This may be done by the help of the following propoſition.

PROPOSITION VI.

If a circle $eDfG$ be deſcribed about the conjugate axis GD, of a given ellipſis ADBG, and a right line EF be drawn, making the given angle $F\pi D$, with

with the conjugate axis, and paſſing through the given point π taken therein, it is propoſed to determine the length of the ſegments F*f*, E*e*, intercepted between the circumference of the circle, and the perimeter of the ellipſis.

From the point F, draw the right line F*d* parallel to the tranſverſe axis AB, meeting the conjugate GD in the point *d*, and the circle in *c*; draw the lines CF, C*f*, C*c*, and let $c\pi$ be joined: Then by conics we ſhall have, as CB : CD :: tang. FπD : tang. $c\pi$D, and in the right lined triangle Cπc, it will be as C*c* (CD) : ſin. Cπc :: Cπ : ſin. C$c\pi$, whence the angle cCπ becomes known; but as CD : CB :: tang. cCπ : tang. FCπ; therefore FCπ is known; from which taking away the given angle fCπ, there remains the angle FC*f*; conſequently all the angles, in the right lined triangle *f*CF, together with the ſide C*f* (CD), are known: we ſhall therefore have, in the right lined triangle, F*f*C, as ſin. *f*FC : C*f* :: ſin. *f*CF : *f*F, one of the required ſegments, and by a ſimilar operation, the other ſegment E*e* will be found, whence as *ef* is given, EF will become known.

COROLLARY I.

The required ſegments F*f*, E*e*, will be found in the ſame manner, when the given point π is not taken in one of the axes, but any where between; but in that caſe, the point where the line EF interſects the conjugate axis, muſt be firſt determined.

COROLLARY II.

If a perpendicular C*n* be let fall from C upon the line EF, the angle πC*n* will be given, to which,

adding F C π (found above) the angle F C n will be known; hence we ſhall have the following analogy for determining Fn: As tang. f C n : tang. F C n :: fn : F n.

Now let KkH (fig. 10.) repreſent the diſk of the ſun, and eDfG that of Jupiter, conſidered as a circle, whoſe diameter is equal to his axis D G, draw Np n, the path of the ſatelles, making the given angle NpR, with a right light Rg drawn parallel to the diameter D G, and let a b be the duration of the eclipſe, and V the apex of the ſhadow in this hypotheſis; join V a, V b, and let the plane a V b be produced, till it meets the ſun's diſk in K and k, it will then interſect the diſk of Jupiter in the line $f\pi e$, and the lines VK, Vk, will alſo touch the circumference of the circle eDfG, in the points e and f, draw the line SV, and it will be the axis of the ſhadow, and conſequently will paſs through C and M, the centers of Jupiter, and the ſection of the ſhadow; join a M, b M, fC, e C, and the triangles abM, efC, will be ſimilar to each other, and, therefore, abM being wholly given, feC will likewiſe be known. Let ADBG be the elliptic ſection of Jupiter's body, and produce $e\pi f$, both ways, till it meets the periphery of the ellipſis in the points E and F, draw KF, k E, and produce them till they meet with a b, produced both ways in N and n, then will N n be the required duration of the eclipſe in the true ſhadow: Now the triangles KfF, KaN, being ſimilar, as are alſo the triangles keE, kbn, and the ſegments Ff, eE, being given by the preceding propoſition, the required ſegments N a, bn, will alſo become known, for they will be to the former ſegments in the given ratio of S M to S C.

 It

It may be obſerved, that this method is equally applicable, whether the axis of Jupiter is perpendicular to his orbit, or not; for if it is not, we can eaſily find by propoſition I and II. the ſpecies and poſition of that elliptic ſection of Jupiter's body, to which a right line connecting the centers of the ſun and Jupiter is perpendicular; and this being obtained every thing elſe will remain as before.

As it would require more time, than I have to ſpare at preſent, to enter into a particular inquiry concerning the alterations, which this irregularity in the ſhadow will occaſion, in the preſent theory of Jupiter's ſatellites, I ſhall conclude with obſerving, that the errors in the ſemi-durations of their eclipſes, ariſing from this cauſe, may ſometimes amount to 20″ in the firſt; 50″ in the ſecond; 2′ 19″ in the third; and 11′ 14″ in the fourth; which errors will, I believe, be deemed ſufficiently large to merit the attention of aſtronomers.

G. Witchell.

Received September 24, 1766.

IV. *An Attempt to account for the universal Deluge, by* Edward King, *Esq; of* Lincoln's-Inn, *F. R. S.*

Read Jan. 22. 1767.

AFTER so many conjectures as have been already formed concerning the cause of the universal deluge, it may perhaps appear both impertinent to attempt a new solution, and also useless, as theories formed on mere hypothesis are always uncertain, and little to be depended upon. But if we give them no more weight than they deserve, and, considering them only as small steps towards the investigation of truth, do not desire any further assent to our conclusions than the probability on which they are founded demands, even such kind of enquiries may be of service, and open a door to new discoveries.

Where we cannot arrive at demonstration we must be content with probability. Our despair of attaining the one ought not to make us neglect the other. And with regard to this remarkable event, the universal deluge, every degree of probability, even the smallest, that appears in an attempt to account for it philosophically, has its use; as it tends to remove those objections that are made to the truth of the fact, by persons who may not think the mere relation of it in the Mosaic writings a sufficient proof of the reality of it; or who may be led, from the difficulty there appears

appears in accounting for ſuch an event, to doubt of the authority of thoſe ſacred books.

Many ingenious hypotheſes have been already formed on this ſubject; but they all ſeem liable to moſt inſuperable objections: and therefore I make no ſcruple to venture another into the world, which appears to me free from ſuch difficulties as they are involved in, and more ſimple. I am willing, however, it ſhould fall to the ground, as ſoon as there appear any reaſonable and weighty objections to it. I only wiſh that the hints contained in this paper may be a means of leading ſome perſon of greater abilities to a more perfect diſcovery; and that it may always be remembered, that the foſſil ſhells found in all parts of the earth, are a ſufficient proof of the truth of its having been at ſome time or other entirely covered with water, however fallible any attempt to account for the deluge may be.

Dr. Burnet, in his theory, has given ſuch an account of the deluge, as Dr. Keill has ſhown to be very improbable, and unphiloſophical. He has firſt deſcribed the primæval earth ſo as to diveſt it of all beauty and elegance, and then has aſcribed the deluge to ſuch cauſes, as are not only ſomewhat inconſiſtent with that part of his theory, where he ſuppoſes the earth to be well watered and moiſtened with dew; but are alſo inſufficient to account for the waters flowing over the tops of the mountains: ſince on the breaking of his imaginary ſhell, it is impoſſible to ſuppoſe that the waters of the abyſs, even on ſuch a concuſſion, ſhould flow up high enough upon thoſe parts that were leſs elevated, ſo as to cover the mountains that now ſubſiſt.

Mr.

Mr. Whiſton has called in the aſſiſtance of another planetary body; and has ſuppoſed the tail of a comet to be ſo greatly condenſed as to afford a quantity of water ſufficient for this purpoſe. But, beſides the inconſiſtency of this theory with that of gravitation, it is no leſs difficult, according to his hypotheſis, to get rid of the water with which the earth was covered, than it is, according to others, to find a ſufficient quantity.

Mr. Ray has accounted for this amazing event, by ſuppoſing a change to have happened in the center of gravity of the earth. But how to find a cauſe for ſuch a change in the center of gravity, and for a reſtoration of it to the ſame place again, is more difficult, and the ſuppoſition of it more inconſiſtent with our philoſophical ideas, than any other hypotheſis whatever.

Such have been ſome of the principal theories hitherto advanced, and far be it from me to preſume that mine may not in the end be found equally fallible; but it appears to me at preſent to be more plain and conſiſtent, and at the ſame time is free from that great difficulty which has perplexed all the reſt, and is indeed the moſt important difficulty in the enquiry, that is, the accounting for a ſufficient quantity of water.

We find in the Moſaic hiſtory of the creation, that God at the firſt created ſea as well as land; and therefore have grounds to believe both from thence, and from the reaſon of things, that there was as great a quantity of ſea on the antediluvian earth, as there is now upon the earth in its preſent ſtate.

We find alſo the whole ſurface of the earth to be undermined by ſubterraneous fires, which make their

appearance

appearance in various places, in very formidable volcanoes. This has been the caſe in Italy, and amongſt the Azores, in Tartary, in Kamtſchatca, in South America, in Ireland, in the iſlands of the Eaſt Indies, and in other parts: and we have reaſon to believe that theſe ſubterraneous fires have made eruptions, not unfrequently, even in the bottom of the ſea; as Mr. Mitchell has made appear in his excellent paper concerning the cauſes of earthquakes*.

We have alſo, in the Philoſophical Tranſactions, an account of entire iſlands being raiſed in the Archipelago, and likewiſe amongſt the Azores, by ſuch ſubterraneous fires †; and Mr. Ray, in his travels, mentions a mountain one hundred feet high, raiſed by the earthquake in 1538, which alſo threw up ſo much earth, ſtones, and aſhes, as quite filled up the Lacus Lucrinus ‡.

To which may be added, that foſſil ſhells and other marine bodies are ſo univerſally found in all parts of the preſent continents and iſlands, as to amount almoſt to a demonſtration, that all the now dry land was once covered with ſea, and that for a conſiderable ſpace of time, probably much longer than the continuance of the deluge is related to have been. For though ſuch a violent flux of waters might have thrown up ſome ſhells and marine bodies upon the hills and mountains, yet it could not have flung up ſuch vaſt quantities, nor ſo univerſally. The prodigious beds of ſhells which we now find in all parts

* Philoſ. Tranſ. Vol. LI. part II. p. 566.

† Philoſ. Tranſ. No 372, or Eames's Abr. vol. VI. part II. p. 203, and Jones's Abr. vol. V. part II. p. 196,

‡ Ray's Travels, old edition, p. 273.

cannot

cannot well be accounted for, but by ſuppoſing the waters, in which thoſe ſhell-fiſh lived, to have covered the countries where they are now found, for a long time, and even for ages.

The ſuppoſition therefore, which I am about to advance, founded on theſe facts, is this; that originally Almighty God created this earth with ſea and land nearly in the ſame proportion as they now remain, and that it continued in that ſtate for many ages, during which the bottom of the ſea became covered with ſhells, and various heterogeneous bodies; that from the firſt of its creation there were alſo many ſubterraneous fires found within the bowels of the earth; and that, at the appointed time, theſe fires burſting forth at once with great violence, under the ſea *, raiſed up the bottom of the ocean, ſo as to pour out the waters over the face of what was before dry land, which by that means became ſea, and has perhaps continued ſo ever ſince, as that which was before the flood the bottom of the ſea, probably from that time has continued to be continent and dry land †.

* Mr. Mitchell has ſhewn, in his paper on the cauſes of earthquakes, that ſuch ſubterraneous fires are at all times very liable to make eruptions under the ſea, and that when they do ſo, the earthquakes conſequent upon ſuch eruptions are more extenſive than any whatever.

† I do not mean by this to inſinuate that all that part of the globe which is now ſea was dry land before the flood: or that the antediluvian ocean was merely of the extent of our preſent continent. I apprehend, on the contrary, that there was always a greater proportion of water on the face of the earth than of continent; and I would only be underſtood to mean, that all that which was dry land before the flood is now buried under the ſea, whilſt that which was a part of the bottom of the antediluvian

This

This hypotheſis may perhaps be liable to great objections; but it is at leaſt conſiſtent with what Moſes relates of the fountains of the great deep being broken up; and, without any perplexity or difficulty, accounts at once for a ſufficient quantity of water to cover the tops of the higheſt antediluvian mountains, even ſuppoſing they were left ſtanding: though it is not improbable but that they might be thrown down by means of the ſame earthquake. If they were left ſtanding, ſome of them might (on the retreat of the waters from their tops after the firſt concuſſion) form ſome of the iſlands that now ſubſiſt.

I muſt alſo add, that this hypotheſis is perfectly conſiſtent with, and perhaps in ſome meaſure accounts for, that ſingular poſition of the ſtrata of coals, ores, and various kinds of earths (mentioned in Mr. Mitchell's paper), which are found always ſloping from mountainous countries, and higher grounds, towards the bottom of the ſea; ſo that what is neareſt the ſurface of the earth in mountains and high countries lies deepeſt in low lands and under the ſea.

It is, beſides, ſomewhat confirmed by that ſingular obſervation of Dr. Haſſelquiſt's, in his travels, (p. 33) where, ſpeaking of Natolia and the eaſtern countries in general, he ſays, "In no place was it more "evident that the continent, we call earth, was in "the beginning the bottom of the ſea." Ulloa alſo informs us, that the ſame thing is evident in the whole country of Valles in South America*: and Norden

ocean forms our preſent land: and that conſequently ſome part of the ocean was ſea both in the antediluvian earth and in the preſent ſtate of it, and common to both.

* Ulloa's voyage to South America, vol. II. p. 99.

 tells

tells us, that the rocks in Egypt bear evident marks of having been washed by the sea *.

These are the reasons which induce me to venture upon this supposition; and now I will just consider one or two objections, that appear to me amongst the most material which may be made to what I have advanced.

It may perhaps be said, that we read † "of the "waters returning from off the earth, and of their "being abated at the end of the hundred and fifty "days: and also, of the waters decreasing continually "till the tenth month; and of the tops of the "mountains being then seen." And it may be objected, that we ought from thence to conclude, that the waters of the deluge, having covered what was before dry ground, afterwards retreated, and left the very same hills and land dry again.

But this conclusion is by no means necessary; for all that can be inferred from what we find in Genesis concerning the decrease of the waters, is, that they gradually subsided from off the face of what is now continent and dry land, as of course they would do on the elevation of it, agreeable to the foregoing hypothesis. And indeed, if the deluge was effected in the way here supposed, we can then give a rational and easy account how all the water came to drain off the ground, and to leave it dry so soon as is recorded; which otherwise, is a circumstance in this place of history very perplexing. It is evident, that such a violent earthquake, or bursting forth of the subterraneous fire, as is here supposed to have raised the

* Norden's Travels, vol. II. p. 21.

† Genesis, ch. viii. 3—5.

bottom

bottom of the then ſea (the preſent continents), at once as high or higher than what was before dry land, muſt in a very ſhort time have drowned and overwhelmed the antediluvian earth, by pouring out the waters upon it; and it is alſo evident, that for ſome time the bottom of the ſea, ſo raiſed, would continue covered with the waters, which, till the vaſt agitation into which they were flung ſubſided, would continue flowing backwards and forwards. But, by degrees, and very eaſily within the time mentioned in Scripture, the water would drain off from all the higher parts, and leave the new land quite dry, and in the ſtate we now find it, with ſtrata of ſhells, and ſand, and ſtones, and other bodies, lying juſt as the ſea had by accident many ages before placed them. Whereas, were the deluge occaſioned only by an addition of water ſufficient to raiſe the ſurface of the ſea higher than the land and mountains, in that caſe, it is impoſſible to imagine any means, at all conſiſtent with the courſe and laws of nature, by which ſuch an immenſe body of water could be evaporated or conveyed away in ſo ſhort a ſpace of time. And beſides, in that caſe, the ſhells, &c. flung upon the land by the concuſſion of the waters, and ſubſiding there within ſo ſhort a ſpace of time, would rather be found lying according to their ſpecific gravities: a fact which Dr. Woodward ſuppoſed certain, but which is by no means true. Nor indeed, according to the conjectures here advanced, is it at all neceſſary that it ſhould be ſo. For, as I imagine the ſhells and other marine bodies, which are now found on various parts of the dry land, to have been placed there gradually during a ſucceſſion of ages, whilſt it was the bottom of the ſea; it will

 follow,

follow, that they muſt be found juſt as the ſea, by its waſhings and motion, laid them; which would of courſe firſt waſh many of them together, and then waſh gravel, or ſand, or clay, or other ſubſtances over them; after which, more ſhells or other bodies would be depoſited, and then more ſtones or gravel, &c. according to the nature of the ſoil. In ſhort, whatever was ſpecifically heavier than water, would (after its removal by any agitation) ſoon ſubſide, and remain fixed, whether the ſubſtances underneath it were ſpecifically heavier than itſelf or no: it is ſufficient that they were but all ſpecifically heavier than the water.

We find to this day great changes are continually making, within the memory of man, both on the face of the earth, in the ſhores, and in the bottom of the ſea, even in thoſe ſmall parts of it that we are acquainted with; and ſuch changes muſt alſo have happened before the flood, and might very probably produce that ſituation of ſhells, &c. ſo different from what might be expected from their ſpecific weights.

Another objection may perhaps be made by ſaying, if all the antediluvian earth was at once overwhelmed, and of courſe all its plants with it, whence came it to paſs, that the now dry land was ſo ſoon covered with vegetables and herbage of all kinds? To this I anſwer, in the firſt place, that the difficulty is juſt the ſame, whether we ſuppoſe the bottom of the antediluvian ſea to be the preſent continents, or whether we ſuppoſe the face of the earth to have remained the very ſame; ſince, by the waters of the deluge, all plants, trees, and vegetables, muſt in both caſes equally have been deſtroyed; and nothing could well remain,

except

except ſome of their ſhoots and ſeeds; which might juſt as well take root on the new continent, on the ſubſiding of the waters, as on the old. And in the next place, I anſwer, that there are not a few inſtances (as is ſhown in Stillingfleet's tracts *) of barren rocks and plains becoming by degrees well covered with verdure, though very remote from any places that might apparently furniſh ſeeds. They have firſt borne a kind of moſs, and afterwards other plants of an higher order (the ſeeds being brought there by accident, and by the various and admirable means of conveyance, which the Creator has given them), till at laſt they have been covered with rich verdure. To which may be added a very extraordinary fact, now well known, namely, that if a piece of ground which has not been cultivated be turned up, and the clods looſened, it will very ſoon produce a variety of plants, ſome of which were never known to grow there before. We find that one acorn is ſufficient to produce a foreſt, and it is by no means to be ſuppoſed (let the deluge have happened how it would) that, immediately after it, the earth was as well cloathed with verdure, as it has become ſince. Probably it was for a time in general very barren, except ſuch parts as Noah and his ſons cultivated, with ſeeds which they had preſerved in the ark.

As to the leaf which the dove brought in †, that might be found on ſome plant which had taken freſh root immediately on the ſubſiding of the waters, or it

* Stillingfleet's Tracts, p. 78, and alſo p. 45, where an inſtance is produced, much to the purpoſe, of marſhes becoming by degrees fine meadows.

† Geneſis, ch. viii. v. 11.

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is not impoſſible but the top of ſome antediluvian mountain, having been but ſlightly covered, might on the ceaſing of the firſt concuſſion (as I before obſerved) remain in the ſtate of an iſland, elevated above the ſurface of the ſea.

I apprehend, no objection of any weight can ariſe from the deſcription of paradiſe in Scripture, nor from its being ſaid that the ark reſted on the mountains of Ararat: ſince, whether the continent was changed or no, there is no place now remaining that anſwers the deſcription of the former; nor is there any thing ſaid about the latter, that ſhould lead us to conclude there ever was ſuch a mountain as Ararat before the flood.

But, leaving theſe objections from the words of Scripture, and the hiſtory of the deluge; another may perhaps ariſe, from this circumſtance, that ſhells are found in various parts of the earth, which are evidently not the ſhells peculiar to the ſeas adjoining, but ſuch as belong to a different climate. This fact at firſt certainly ſeems to contradict what I have advanced: and yet, when well conſidered, it will perhaps rather be found to confirm my hypotheſis. For let any one but look on a terreſtiial globe, and he will inſtantly ſee, that the preſent continents are evidently not in the ſame climates as the preſent ſeas; and therefore, though the ſhells found in many places of the earth are not found in the neighbouring parts of the ocean; yet, when thoſe parts of the earth were ocean, they might have had a very proper climate and ſituation there. Thus, for inſtance, we may obſerve that the Mediterranean is in a more ſouthern climate than the neighbouring continent of Europe, and in a more northern climate than that of Africa. And the whole

whole continent of Aſia is in a climate much more northern, than the neighbouring Indian ocean.

But, if this ſolution of the difficulty is not thought ſufficient, it may be added, that ſo great a concuſſion, and ſuch a change in the figure of the earth, as muſt have happened from the ſubterraneous fires elevating ſo many parts higher than they were before, might poſſibly affect the gravitation of the parts of the globe of the earth, and cauſe it to revolve round a different axis after the flood; whence there would undoubtedly ariſe a change of climate in all parts, ſufficient to account for the preſent ſituation of ſhells, in places ſo foreign to the climates where ſhell fiſh of the ſame ſpecies are now found. And as I have before obſerved with regard to ſeeds, ſo it may alſo be obſerved with regard to ſhell fiſh, that the conveyance of a very few of each ſort (by the flux of water) to the beds proper for them, would be ſufficient to preſerve all the various kinds, and to cauſe them now to be found in ſuch numbers, in thoſe parts of the ocean that are beſt adapted to each peculiar claſs.

Another thing proper to be taken notice of, is the horns and bones of terreſtrial animals being found in the earth, together with foſſil ſhells; which ſeems to contradict the ſuppoſition of the preſent continents having been originally the bottom of the ſea. But with regard to this, I muſt beg leave to obſerve, that probably ſome of thoſe bones have been depoſited there ſince the flood, and have been covered by an addition of earth, as has happened alſo to ſome of the trees and woods that were cut down in this iſland by the Romans. And, as to the reſt, it cannot be

be ſuppoſed, but that on the firſt great eruption, which poured the waters of the ocean upon the dry land, there muſt have been a violent agitation for ſome time, by their flowing backward and forward; during which interval, the bodies of many terreſtrial animals (floating on the water) would be waſhed to different parts of the new-raiſed continent, and be left there as the water ſubſided.

Some little objection perhaps may ariſe, from its being obſerved, that the ſea at preſent covers a much greater part of the globe than the dry land does.

But I apprehend this was alſo the caſe before the flood; and it may eaſily be conceived, that ſome part of the bottom of the antediluvian ocean might be flung in the manner ſuppoſed in this paper, and not the whole; and that the bottom of the preſent ocean conſiſts not only of what was before the flood dry land, but alſo of ſome part of what was, even from the beginning, the bottom of the ſea.

I will therefore only juſt add, that probably the ſame ſubterraneous fires (which originally raiſed the continents and iſlands that now appear, and have ever ſince been making great changes in the bowels of the earth, and producing thoſe tremendous earthquakes, which have happened from time to time) may in the end break forth with redoubled violence, and deſtroy it, in the manner foretold in Scripture.

It may not be amiſs to add, in confirmation of the foregoing hypotheſis, that the beds of ſhells, diſcovered in chalk pits, gravel pits, and other places, conſiſt generally of one or two, or at moſt of a very few

few different ſorts in each particular place, as they would of courſe do upon a ſuppoſition that thoſe reſpective beds were formerly at the bottom of the ſea, in the ſeveral places where thoſe different kind of ſhell fiſh lived and bred; and that they were from thence, together with the bottom of the antediluvian ocean, raiſed up by the force of ſubterranean fires: for we may obſerve in the preſent ſeas, that one ſpecies of ſhell fiſh take up their habitation in one place, whilſt thoſe of a different ſpecies are found in ſome other; and that numbers of the ſame kind, as for inſtance cockles, or oyſters, are generally found on the ſame banks. The preſent appearance of foſſil ſhells, therefore, does at leaſt in this reſpect ſeem conſiſtent with the conjectures here advanced: whereas, upon a ſuppoſition that theſe foſſil ſhells were carried to their reſpective places, at the time of the flood, merely by the torrent of water that then flowed to and fro, they ought rather to be found mixed promiſcuouſly together; and not thoſe of one ſpecies in one place; and thoſe of a different ſpecies in another. And I beg leave here to mention, that, ſince the writing of the foregoing paper, I find an hypotheſis ſomewhat ſimilar to what is here advanced was adopted by Lazzaro Moro, a Venetian author, who aſſerts that the continents were originally raiſed by ſubterranean fires; but he conſiders this merely as the cauſe of their firſt and original formation, and not as having occaſioned the deluge, nor as having happened at that time.

Received September 24, 1766.

V. *An Attempt to account for the formation of Spars and Cryſtals.* *By* Edward King, *Eſquire, of* Lincoln's Inn, *F. R. S.*

Read Jan. 29, 1767.

IN all our enquiries concerning the works of the Supreme Being, and in our endeavours to inveſtigate the ſecondary cauſes by which the various phænomena of nature are effected, we certainly cannot be too cautious and circumſpect. Our comprehenſion of things is at beſt very imperfect; and we may eaſily be led, by too haſty a view, to draw falſe concluſions. It therefore becomes us to be very modeſt, and to endeavour to go upon ſure grounds, before we indulge any hypotheſis whatever. But if we obſerve this rule, we cannot carry our ſearches or conjectures too far, ſince the reſult of them will often be the diſcovery of what may be uſeful to mankind, and will almoſt always increaſe our knowledge in ſome degree, and be a means of raiſing in our minds more noble and exalted ideas of the Almighty Creator of all things.

This conſideration encourages me to fling together the following conjectures, with regard to the production of thoſe beautiful and tranſparent ſubſtances, which go by the name of ſpars, cryſtals, and jewels; but I would not venture to conſider them as amounting to any thing more than queries.

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I was firſt led to entertain theſe thoughts, by obſerving the nature of Briſtol ſtones, and the various ſtates in which they appear; and was encouraged to think they might have ſome little weight, by obſerving alſo the nature of ſpars, and of thoſe ſtony concretions found in large caverns. I ſhall, therefore, firſt mention the obſervations I made, and then draw my concluſions.

1. And in the firſt place, it is known, that the Briſtol ſtones grow within the hollow cavity of ſome other rough ſtone; and that the ſubſtance of the external ſtone is porous, and frequently ſo ſtrongly impregnated with cryſtalline corpuſcles, that they glitter amongſt the earthy particles, when held up to the light.

2. In the next place, it is to be obſerved, that, wherever there is a hollow cavity in theſe kind of ſtones, the inſide is almoſt always lined with ſuch ſhining ſubſtances, either in a perfect or imperfect ſtate.

3. We find the Briſtol ſtones appear in ſeveral different ſtates; for in ſome places of the cavity, where the cryſtallization is not completed, they are of a duſky red, without any tranſparency; in others they appear of a dirty yellow; and in others white; and at laſt tranſparent.

4. As to the ſpars and cryſtals formed even in flints, and other hard bodies; I think they are generally obſerved in ſuch as have evidently been at one time or other in a ſoft ſtate, and lay in or near moiſt places ſtrongly impregnated with ſaline particles of ſome kind or other; or elſe they are found in bodies wherein ſome ſaline and moiſt ſubſtances have for-

merly been inclofed, and prevented from evaporating; of which kind are the fpars found in foffil fhells, wherein the bodies of the fhell fifh have perhaps lain and perifhed.

5. We obferve, not only in the fmall cavities of ftones, but alfo in large caverns, fuch as thofe in the Peak in Derbyfhire, Okeyhole in Somerfetfhire, and the famous grotto in the Greek ifland of Antiparos, and in fhort wherever moifture defcends through the earth to a void fpace, and ftops upon the inward furface, that it there forms cryftals, or fpars, or ftony concretions of fome fort or other; of which fome are fo very imperfect, as to have only the appearance of rude heaps of petrified matter, without any regular form, which chiefly happens where there is much moifture, and where it defcends, or foaks through pores fo large as to carry many earthy particles with it.

6. To all which I muft add, that Sir Ifaac Newton has made it appear, that the tranfparency of bodies is occafioned by the minutenefs of their pores, and the opacity of them by the largenefs of the pores, in which the rays of light being reflected from fide to fide are loft, and prevented from paffing through; whence it is, that paper becomes tranfparent by being oiled, and the oculus mundi ftone by being foaked in water.

Thefe are the principal obfervations on which I found my conjectures; and from hence I am induced to conclude, that all thefe above-mentioned fubftances, are formed by means of thofe cryftalline (perhaps faline) corpufcles with which the furrounding earth or porous ftones abound, and which probably are diffufed

diffufed throughout the whole globe, and mixed in fome degree with moft ftratas. Thefe fmall particles, I apprehend, are carried along gradually, by the moifture, or vapors, which foak through the pores, till they come to fome cavity, and there, being ftopped by the difcontinuance of the earthy or ftony fubftance from proceeding any further, they collect together in drops, and as they dry and harden, do of courfe, by their mutual attraction, form themfelves into cryftalline figures; and as the pores are more and more filled up, by the acceffion of more corpufcles, or by their mutual attraction which draws them clofer together, they become more and more tranfparent. Some, however, of the bodies thus formed never have any tranfparency at all, being mixed with too many earthy or ftony particles, or other heterogeneous matter, and have fometimes fo much of that as not to be able to put on any regular form, but only to petrify in a confufed heap; the earthy or ftony particles preventing the cryftalline or faline particles from forming themfelves, by their mutual attraction, into regular figures; and there being perhaps but few of the true cryftalline corpufcles mixed with them. This feems to be the cafe with many of the ftony concretions in large caverns: and perhaps, from a fmall mixture of thefe fame heterogeneous particles it is, that fpars are inferior to cryftals, and alfo differ from one another. Mr. Platt, in the Philofophical Tranfactions, Vol. LIV. p. 41. has obferved, that fpar feems to be nothing but cryftal debafed by a calcareous earth.

I cannot help fufpecting, that what I have called cryftalline corpufcles are in reality a kind of falts; I will beg leave, therefore, to call them hereafter by

that name; and will juſt endeavour to illuſtrate what I have ſaid more particularly by the inſtance of Briſtol ſtones. In their firſt ſtate, theſe are of a dirty red, or ſome other duſky color; but afterwards, as more ſalts, or cryſtalline corpuſcles, are added, by the deſcent of moiſture, or the paſſage of more vapor, they begin to be more compact; and then, the pores becoming ſmaller, they approach nearer to tranſparency, and put on a yellow or whitiſh color; and at laſt, receiving a further addition of ſalts, and having the component particles drawn ſtill cloſer together by their mutual attraction, they become ſtill harder and more tranſparent, till they acquire, by a length of years, their greateſt degree of perfection.

As to the Briſtol ſtones being found of ſuch different ſizes; I am induced to think they grow larger or continue ſmall, only by the accident of the moiſture bringing ſalts to them faſter or ſlower; for had they any other regular method of growing, I think they ought never to be found in their moſt perfect ſtate, till they had firſt acquired their full bulk; whereas, on the contrary, they are found in their greateſt degree of perfection of all ſizes.

I therefore imagine, that, till the outſide ſurface is hardened, whatever addition of ſalts is made will increaſe the bulk of the ſtone; but that, after the outward ſurface is once hardened, the addition of ſalts then only helps to bring it to its moſt perfect tranſparent ſtate; and therefore, that when the flow of moiſture or vapor, and conſequently the addition of ſalts, is very quick, there (the outward ſurfaces not having time to harden till a conſiderable quantity of ſalts are accumulated together) the ſtone, will grow

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large; but where the flow of moiſture is ſlow, there (the outſide ſurfaces, and indeed the whole maſs, becoming hard before a freſh ſupply of ſalts is added) the ſtones will be ſmall. And again, where the moiſture and ſalts paſs through large pores, there the cryſtal, or ſpar, or other concretion, will be very imperfect, being mixed with much heterogeneous matter; but where the moiſture, and cryſtallizing particles, or ſalts, deſcend though very minute pores, there the ſalts will be moſt unmixed and pure, and the cryſtal or ſtone will be of the moſt tranſparent kind. In ſhort, I cannot but look upon the largeſt caverns in the earth, and the ſmalleſt cavities in ſtone, as producing ſimilar effects, and therefore conſider them in the ſame light.

In theſe concluſions I may perhaps be miſtaken, but they are at leaſt conſiſtent with the obſervations we make: for wherever there are cavities in the earth, or in ſtones, into which moiſture can any way deſcend, we almoſt always find theſe kind of cryſtallizations and concretions; and the more plentiful the moiſture is, and the more porous the ſtrata of earth or ſtones are through which it paſſes, the larger the concretions are, and the more remote from a tranſparent ſtate; as appears in thoſe great caverns in the Peak, and in Somerſetſhire, &c. Whereas, on the contrary, the harder and leſs porous the ſubſtance is through which the moiſture paſſes, the more tranſparent are the ſtones formed by it, as in the caſe of Briſtol ſtones, and of ſome of thoſe beautiful ſpars adjoining to veins of ore.

Whether all kind of ſtones may not be formed in ſomewhat the ſame manner, by the water carrying the

the ſtony particles to the ſame place, and their collecting there together, by their mutual attraction, I leave to others to determine; but I am much perſuaded, that this may probably be the manner of the production of ſpars and cryſtals: and perhaps jewels, or precious ſtones, may grow juſt in the ſame way; and owe their perfection ſolely to their being compoſed of ſtill more minute ſalts, and more ſlowly; whence we may conjecture, why it is ſo rare to find large diamonds. I have obſerved ſome of the Briſtol ſtones to have a fine purple appearance, like an amethyſt; and it is well known, that ſeveral ſorts of ſpars are of various beautiful colors, by means of a mixture of mineral particles, in which they have a diſtant reſemblance of jewels; and indeed they ſeem to be very analogous to them in many reſpects.

After all, however, I am ſenſible that what I have advanced deſerves not to be conſidered as any thing more than mere gueſs. I know that a confirmation of the truth of theſe conjectures muſt depend upon experiments, which I have it not in my power to make: and I can only conclude with wiſhing, that ſome gentlemen, converſant in chemiſtry, may ſome time or other attempt to analyze theſe bodies, and to ſee whether they really are compoſed of what may properly be called ſalts, and of what kind.

It is much to be lamented, that, in enquiries into the nature of foſſils, there have not been more chemical experiments made.

Received October 10, 1766.

VI. *Experiments with Camphire, by Mr.* Alexander, *Surgeon in* Edinburgh.

Read Jan. 29, 1767.

AS medical authors have differed fo widely in their opinions concerning the nature and effects of camphire, one part of them pofitively affirming that it heats, and another afferting with the fame confidence that it cools the body; I made the following experiments with it, in order, if poffible, to have cleared up the difficulty.

If camphire was a heater, I concluded it would raife my pulfe, and augment my natural heat; and therefore, previous to my taking it, I counted the number of pulfations in a minute, which were fixty-eight, and found that, in the fpace of five minutes, the mercury in Fahrenheit's thermometer arofe eighteen degrees by the heat of my ftomach *.

Having thus found the ftate of my pulfe, and of my natural heat; I took ℈j of camphire in a little of the pulp of tamarinds; and twenty minutes afterwards applied the thermometer to my ftomach: the mercury, in the fpace of five minutes, arofe exactly eighteen degrees, as it had done before taking the dofe, but my pulfe beat only fixty-fix, which was two ftrokes lefs.

Three quarters of an hour after I had taken the camphire, I applied the thermometer again; in the

* The thermometer was applied to the pit of my ftomach.

fame fpace of time, the mercury arofe exactly the fame as in the laft trial, but my pulfe beat only fixty-five which was one ftroke lefs, and three fewer than it had done before I took the camphire.

The next day, having found that the mercury arofe nineteen degrees in five minutes, by the heat of my ftomach; and that my pulfe beat feventy-feven in a minute, I took ℈ij of camphire in a little of the fyrup of pale rofes: immediately after fwallowing it, I felt a fenfation in my mouth fomething fimilar to that occafioned by ftrong peppermint-water, but much more difagreeable; ten minutes after I had taken it I applied the thermometer to my ftomach; in five minutes the mercury arofe eighteen degrees, which was one degree lefs than it had done before I had taken it. My pulfe now beat only feventy, whereas before I took the dofe it had beat feventy-feven: five and twenty minutes after I had taken it, I applied the thermometer again, and the mercury arofe the fame as at the laft trial, but my pulfe had increafed from feventy to feventy-feven, the exact number which it had beat before I took the camphire: foon after this, my head grew fo very giddy that it was with great difficulty I could walk through the room. In this condition, I had an inclination to breathe the frefh air, opened the window and looked over into the ftreet, where every thing appeared to me in the utmoft tumult and confufion; feeling myfelf in danger of tumbling from my ftation, I fhut the window and ftaggered from it to bed, threw myfelf down upon it, and having a book with me, endeavoured to read, but had no diftinct idea of any one fentence, and far lefs could I connect two or more of them together fo as to comprehend the meaning of the author:

author: not being able to amuſe myſelf by reading, I aroſe, to ſee whether I could walk any better, but, to my great mortification, found that I was more giddy and could hardly walk at all. I then returned to the bed, and feeling myſelf thirſty, called for ſome mutton broth; it being dinner-time, the ſervant, inſtead of bringing the broth, covered the table as uſual, not knowing that I was any way diſordered. Seeing the dinner on the table, I got out of bed again, and with no ſmall reluctance, ſwallowed down a plate-ful of the broth, but could neither taſte bread nor meat, on account of a nauſea, which howevever was not accompanied with any inclination to vomit.

I now ſtaggered again to the bed, and took up the book I had left there, with a deſign to divert the attention of my mind into ſome other channel than that into which the preſent confuſion of my ideas had hurried me: at this time, ſelf-preſervation ſuggeſted to me the expediency of taking a vomit; but as I felt very little pain, and was not apprehenſive of much danger, I reſolved not to ſpoil the ſucceſs of my experiment by evacuating the camphire before I ſhould diſcover what its effects would be. Hitherto, amid a tumult of indigeſted ideas, I had retained ſome degree of ſenſibility; but now there aroſe ſuch a noiſe in my ears, the confuſion and giddineſs of my head increaſed ſo much, that all conſciouſneſs of what was preſent, as well as memory of what was paſt, were ſoon entirely obliterated, ſo that whether I endeavoured to read in the book I had taken up, or what elſe I did, I know not.

Fortunately, at this juncture, one of my young gentlemen came into the room, who told me, after I

 recovered,

recovered, that I defired him to fhut the windows, and threw myfelf backward on the bed, where I lay a few minutes very quiet, after which, in a fort of frenzy, I ftarted up and fat upon the fide of it, made fome efforts to vomit, but evacuated nothing; that I then threw myfelf back again, fell into ftrong convulfions, foamed at the mouth, fhrieked with great violence, ftared dreadfully at, and endeavoured to grafp and tear every thing around me. This outrageous fit was fucceeded by a calm, fomething fimilar to fainting, during which time a relation was fent for, who came between three and four o'clock; when he fpoke to me, I awaked, as I thought, from fleep, and knew him, though almoft intirely infenfible to every other object. Soon after, came Dr. Cullen, who had been fent for alfo; when he had felt my pulfe, which beat one hundred in a minute, he ordered me to be blooded; but as it is probable that natural antipathies will remain when every other fenfation is nearly loft, I obftinately refufed to undergo this operation, on account of an unfuperable averfion I have to it. All this time, no perfon knew any thing of my having taken the camphire, nor did I recollect any thing of it myfelf; and though I was recovered fo much from the fit I have juft now defcribed, as to know every one about me, I neither knew where I was, nor what I did.

As I felt a very uncommon fenfation of heat, I got violently out of the bed, and threw myfelf on the floor, the coolnefs of which was very agreeable to me; upon which fome cold water was brought, and my hands and face bathed in it; this proved ftill more agreeable, and in fome degree quieted a tremor which had feized on every part of my body. At this time, Dr.

Dr. Monro, junior, who had alſo been ſent for, came to my aſſiſtance. As I could give him no account of the cauſe of my illneſs; while he was walking through the room, he accidentally caſt his eyes on a paper I had left on the table, containing the relation of my having taken the camphire, and its effects upon me, ſo long as I had been able to mark them. Upon this diſcovery, he immediately ordered me warm water; of which having drank pretty plentifully, I ſoon vomited, and, though more than three hours had paſſed, ſince I had taken the camphire, a great deal of it was evacuated in an undiſſolved ſtate.

While I was holding my head over the baſon into which I was vomiting, the ſmell of the camphire aroſe very ſtrong, and firſt made me recollect that I had taken it, though I could give no diſtinct account of the time when, or manner how. I now, by the Doctor's order, drank the juice of two or three lemons and oranges, but was not ſenſible of any benefit from them. I mentioned before, that I had not only loſt all remembrance of my paſt actions, but alſo the knowledge of almoſt every preſent object; but I now began, in ſome degree, to recover both; though in a manner ſo extraordinary, as I cannot poſſibly deſcribe, ſo as to give a clear idea of it. Among the firſt things I recollected, was, that I had that day viſited ſeveral patients; but I could neither diſcover their diſeaſes, names, number, nor any other circumſtance relating to them. I could likewiſe recollect, that I had formerly known a great many things, of which I was become intirely ignorant, but could not fall on any method of recovering that knowledge which I had loſt. A perſon

ſon who has loſt his ſenſes by liquor, as ſoon as he recovers, is perfectly well acquainted with every thing he knew before: but the caſe was very different with me, for the furniture of my room, and almoſt every other object on which I caſt my eyes, appeared as ſtrange, and new to me, as if I had only that moment begun my exiſtence; and though I could remember the name of any thing when I looked at it, yet it was not without inveſtigating its nature, that I could diſcover its uſe.

I had been put to bed when I vomited, and I know not whether it was owing to it, or the camphire, but I had now a ſevere head-ach, which diſturbed me not a little all the evening. Between five and ſix o'clock I aroſe, and drank a bowl of tea, and the diluted juice of ſome more lemons and oranges. The giddineſs in my head, ſinging in my ears, exceſſive heat and tremor, which I had felt ſo ſeverely before, were now conſiderably abated, though far from being intirely gone off. About ſeven o'clock, I had another viſit from Dr. Monro, who, upon numbering my pulſations, found they were now reduced from one hundred to eighty; in a minute after this, the thermometer was applied to my ſtomach, and in half an hour the mercury aroſe two degrees above blood-warm; it was then removed from my ſtomach to the Doctor's, and the mercury fell more than one degree.

Between eight and nine o'clock, though I was conſiderably better, I ſtill felt an uneaſineſs of body, and a confuſion of mind, which it is impoſſible to deſcribe; on account of which, I went to bed, and very ſoon fell into a calm and ſoft repoſe, which continued,

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continued, without any interruption, till next morning. When I awaked, I found my head-ach quite gone, though a ſmall degree of the confuſion in it ſtill remained. Upon going to ſtool that morning, I was extremely coſtive, though I had not been ſo before, nor continued to be ſo after. All that day I felt a great ſoreneſs, and rigidity over my whole body, as if I had caught cold, or undergone ſome ſevere exerciſe; the next day I was ſomething better, and the day following quite recovered.

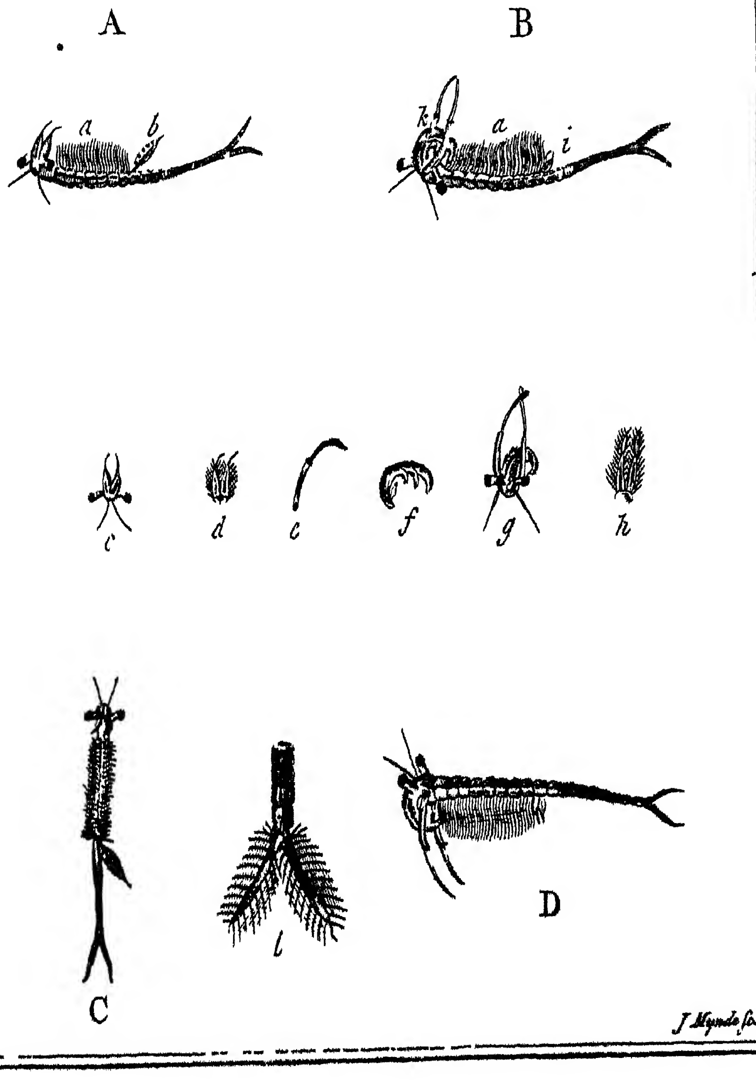
As the foregoing experiments had not fully ſatisfied me, whether camphire acted as a heater or cooler on the body, I reſolved to try if it would give any additional heat or cold to fluids, in which it was diſſolved; but, after repeated trials, I found that it never altered the natural heat of ſpirits, or oils, in whatever degree they were impregnated with it.

The firſt doſe I took was a moderate one, and appeared to have acted as a cooler; but the next, if there is any truſting to the ſenſations occaſioned by it, or to the increaſed celerity of the blood, certainly muſt have heated to a very great degree.

VII. *A Defcription of a very remarkable aquatick Infect, found in a Ditch of ftanding Water near* Norwich, *in the Spring of the Year*, 1762. *By* Edward King, *Efquire, of* Lincoln's-Inn, *F. R. S.*

Read Feb. 5, 1767.

TAB. IV. A is the female, and B the male, both reprefented on their backs, in the pofture in which they ufually fwim; *a*, *a*, are a number of fmall tranfparent, fringed, fins, placed parallel, and contiguous to each other. They are almoft always in a waving motion, and the animalcules feem to keep themfelves fufpended, at different heights, in the water, by means of them; for on their ceafing to move they fink to the bottom: *d* is one of thofe fins belonging to the female feen in front, and *b* is one of thofe belonging to the male, in which there is a very remarkable difference: *c* is the head of the female; and *g* is the head of the male; diftinguifhed by three projecting fubftances like horns, or tufks which are marked *k* in figure B; one of the long ones on the fide is drawn feparate at *e*, and the crooked one in the middle at *f*; this laft probably ferves as a kind of trunk, and the former may be of fervice to catch their prey, whatever it is; *i* is a very fingular projecting fubftance in the male, and may, perhaps, contain the parts of generation; and *b* is the ovarium of the


A
a
b
B
k
a
i
c
d
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the female, in which (it being quite tranfparent) the ova, or fpawn, are very vifible, and may be feen, from time to time, to change their places, and to have a kind of circulation.

C is a view of the female, placed on its back, in order to fhew the pofition of the fins, and their appearance when one looks down upon the infect; and D is the male, placed with his back uppermoft, in the pofture in which he fometimes lies ftill at the bottom of the water. Laftly, *b* is the tail magnified in a microfcope, fhowing the hairs which grow on both fides out of it; but as the animalcule did not lie ftill long enough in the water, I could not view it with a glafs fo exactly as I wifhed to do, and, therefore, am not fure of the accuracy of the drawing of this part; all the other parts I have drawn as carefully as I was able, and they are about the natural fize.

In thefe infects, befides their form, feveral particulars are very remarkable. 1. Their bodies are intirely tranfparent, and moftly of a yellowifh hue, except towards the tail, and part of the ovarium, where the color is reddifh; and, through a long veffel, which reaches almoft the whole way, from the head to the tail, fomewhat of a circulation, by fits and ftarts, is very vifible, even to the naked eye. 2. In the ovarium of the female, the ova (which are of a mixed color in different parts, fome brown, fome yellow, and fome red) are alfo in a conftant circular motion round the bag (or at leaft, by a deception of fight, they appear to be fo). 3. They fwim conftantly on their backs, keeping themfelves fufpended by the vibrations of their numerous fins, and moving forwards, by giving a fudden fpring with

their tails; which latter circumſtance is common to almoſt all aquatick inſects.

In the ditch from whence theſe were taken, there were a vaſt multitude of the ſame kind, though they have not been found in any other place that I know of. From their being prolifick in this ſtate, I ſuſpect it to be their only one, and that they are merely aquatick, and never turn to flies, as many inſects found in water do: but then it ſeems very unaccountable, how they came to be in ſuch abundance in this ditch, and no where elſe, at leaſt ſo as to be obſerved.

They were diſcovered by a poor man, now dead, whoſe genius was very extraordinary, and much ſuperior to what is uſually found in his rank of life. He was indefatigable in his ſearches after every thing curious in nature, and, without ever having had any advantages of education, had acquired a degree of knowledge by no means contemptible. He kept a great number of theſe inſects for a conſiderable time, and they ſeemed to receive their nouriſhment chiefly from the water itſelf, or from the moſt minute animalcules in it; not being perceived to feed on any thing that could be taken notice of. I had ſeveral of them for ſome days in my cuſtody, from which I took this drawing; and they were ſeen and obſerved by many perſons.

VIII. *An Account of the very tall Men, ſeen near the* Streights *of* Magellan, *in the Year* 1764, *by the Equipage of the* Dolphin *Man of War, under the Command of the Hon. Commodore* Byron; *in a Letter from Mr.* Charles Clarke, *Officer on board the ſaid Ship, to* M. Maty, *M. D. Sec. R. S.*

Weathersfield, November 3, 1766.

SIR,

Read Feb. 12, 1767.

I Had the pleaſure of ſeeing my friend Mr. M—— a few days ago, when he made me acquainted with your deſire of a particular account of the Patagonians, which I moſt readily undertake to give, as it will make me extremely happy if I can render it in the leaſt amuſing or agreeable to you. I wiſh I could embelliſh it with language more worthy your peruſal; however, I will give it the embelliſhment of truth, and rely on your goodneſs to excuſe a tar's dialect.

We had not got above ten or twelve leagues into the ſtreights of Magellan, from the Atlantic ocean, before we ſaw ſeveral people, ſome on horſeback and ſome on foot, upon the north ſhore (continent), and with the help of our glaſſes could perceive them beckoning to us to come on ſhore, and at the ſame time obſerved to each other that they ſeemed of an

 extraordinary

extarordinary ſize; however we continued to ſtand on, and ſhould have paſſed without taking the leaſt farther notice of them, could we have proceeded, but our breeze dying away, and the tide making againſt us, we were obliged to anchor, when the commodore ordered his boat of twelve oars and another of ſix to be hoiſted out, manned and armed. In the firſt went the commodore, in the other Mr. Cummings our firſt lieutenant and myſelf. At our firſt leaving the ſhip, their number did not exceed forty; but as we approached the ſhore, we perceived them pouring down from all quarters, ſome galloping, others running, all making uſe of their utmoſt expedition. They collected themſelves in a body, juſt at the place we ſteered for. When we had got within twelve or fourteen yards of the beach, we found it a diſagreeable flat ſhore with very large ſtones, which we apprehended would injure the boats; ſo looked at two or three different places, to find the moſt convenient for landing. They ſuppoſed we deferred coming on ſhore, through apprehenſions of danger from them, upon which they all threw open the ſkins which were over their ſhoulders, which was the only cloathing they had, and conſequently the only thing they could ſecret any kind of arms with, and many of them laid down cloſe to the water's edge. The commodore made a motion for them to go a little way from the water, that we might have room to land, which they immediately complied with, and withdrew thirty or forty yards; we then landed, and formed each man with his muſquet, in caſe any violence ſhould be offered. As ſoon as we were formed, the commodore went from us to them, then at about twenty yards

diſtance; they ſeemed vaſtly happy at his going among them, immediately gathered round him, and made a rude kind of noiſe, which I believe was their method of ſinging, as their countenances beſpoke it a ſpecies of jollity. The commodore then made a motion to them to ſit down, which they did in a circle with him in the middle, when Mr. Byron took ſome beads and ribbons, which he had brought for that purpoſe, and tied about the women's necks, &c. with which they ſeemed infinitely pleaſed. We were ſtruck with the greateſt aſtoniſhment at the ſight of people of ſuch a gigantic ſtature, notwithſtanding our previous notice with our glaſſes from the ſhip; their number was increaſed by the time we got on ſhore to about five hundred, men, women, and children. The men and women both rid in the ſame manner; the women had a kind of belt to cloſe their ſkin round the waiſt, which the men had not, as theirs were only flung over their ſhoulders, and tied with two little ſlips (cut from the ſkin) round the neck. At the time of the commodore's motion for them to retire farther up the beach, they all diſmounted, and turned their horſes looſe, which were gentle and ſtood very quietly. The commodore, having diſpoſed of all his preſents and ſatisfied his curioſity, thought proper to retire, but they were vaſtly anxious to have him go up into the country to eat with them; (that they wanted him to go with them to eat, we could very well underſtand by their motion, but their language was wholly unintelligible to us.) There was a very great ſmoke to which they pointed, about a mile from us, where there muſt have been ſeveral fires; but ſome intervening hills prevented our ſeeing any thing but the

the ſmoke. The commodore returned the compliment, by inviting them on board the ſhip, but they would not favour him with their company, ſo we embarked and returned to the ſhip. We were with them near two hours at noon day, within a very few yards, though none had the honour of ſhaking hands but Mr. Byron and Mr. Cummings; however, we were near enough and long enough with them to convince our ſenſes ſo far as not to be caviled out of the very exiſtence of thoſe ſenſes at that time, which ſome of our countrymen and friends would abſolutely attempt to do. They are of a copper colour, with long black hair, and ſome of them are certainly nine feet if they don't exceed it. The commodore, who is very near ſix foot, could but juſt reach the top of one of their heads, which he attempted, on tip toes, and there were ſeveral taller than him on whom the experiment was tried. They are prodigious ſtout, and as well and proportionally made as ever I ſaw people in my life. That they have ſome kind of arms among them is, I think, indiſputable, from their taking methods to convince us they had none at that time about them. The women, I think, bear much the ſame proportion to the men as our Europeans do; there was hardly a man there leſs than eight feet, moſt of them conſiderably more; the women, I believe, run from $7\frac{1}{2}$ to 8. Their horſes were ſtout and bony, but not remarkably tall; they are in my opinion from 15 to $15\frac{1}{2}$ hands. They had a great number of dogs, about the ſize of a middling pointer, with a fox noſe. They continued upon the beach till we got under way, which was two hours after we got on board; I believe, they had ſome expecta-

tions

tions of our returning again; but as ſoon as they ſaw us getting off, they betook themſelves to the country.

The country of Patagonia is rather hilly, though not remarkably ſo. You have here and there a ridge of hills, but no very high ones. We lay ſome time at Port Deſire, which is not a great way to the northward of the ſtreights, where we traverſed the country many miles round; we found firebrands in different places, which convinced us there had been people, and we ſuppoſe them to have been the Patagonians. The ſoil is ſandy, produces nothing but a coarſe harſh graſs, and a few ſmall ſhrubs, of which Sir John Narborough remarked, he could not find one of ſize enough to make the helve of a hatchet, which obſervation we found very juſt. It was ſome time in december we made this viſit to our gigantic friends. I am debarred being ſo particular as I could wiſh, from the loſs of my journals, which were demanded by their lordſhips of the admiralty, immediately upon our return; but if any article is omitted which you are deſirous of being acquainted with, I beg you will take ſome means of letting me know it, for I will moſt readily communicate every circumſtance of the matter, that fell under my obſervation, as it is with the greateſt pleaſure and reſpect that I ſubſcribe myſelf,

S I R,

Your very humble ſervant,

Charles Clarke.

IX. *A Letter from Mr.* William Sharp, *Surgeon to St.* Bartholomew's *Hospital, to* James Parsons, *M. D. F. R. S. containing an Account of a new-invented Instrument for fractured Legs.*

S I R,

Read Feb. 12, 1767.

AS the following treatment of fractured legs (from the experience I have had of its success during a practice of several years) appears to me preferable to any I have hitherto known, and as it may be a means of lessening many of the inconveniences attending such accidents, I take the liberty of sending it for your opinion; and, if you think it of consequence enough to be made public, shall be glad to have it laid before the Royal Society.

The instrument here recommended was first applied with great success in an oblique fracture of the tibia (which could not be kept in a proper situation by the usual methods), and afterwards, as happily, in a dislocation of the lower extremity of the same bone, accompanied with a fracture of the fibula. In this latter case, it is often difficult to reduce the dislocation even with a strong extension, and more so to retain the bones in their proper situation, while the limb is laid in the usual extended posture. But both these difficulties are absolutely avoided by the means I am about to describe.

The

The remarkable good effects in the cases above-mentioned, induced me to try the same in different fractures of the leg, as well compound as simple; in all which I have found it to answer my expectation.

I have formerly explained this method of treating fractures to many gentlemen of the profession, as well as to yourself; and have also had several cases where other surgeons have been concerned with me, who have in general expressed great satisfaction (and some of them have introduced it into their own practice); so that I have reason to believe it would have become more general, if the instruments, that have hitherto been sold for that purpose had been made according to the original pattern; but the workman, whom I employed, has made and sold many that differ from mine in some essential points. I have therefore thought it necessary to send you, inclosed, a description of the instrument, made after such a manner as I have found by experience to succeed best.

I am, with great esteem,

SIR,

Your most obedient, and

most humble servant,

Mincing-Lane,
Nov. 10, 1766.

W. Sharp.

Defcription of a new invented Inftrument for fractured Legs, (confifting of two Parts, which at prefent I fhall call upper and under fplints) recommended to be ufed inftead of the common Apparatus. (See the Plate.)

THE figures are drawn on a fcale of three inches to a foot, and reprefent two fplints of ftrong pafteboard, made with glew, to be faftened upon a fractured leg, by three ftraps which furround the whole.

Thefe are adapted to the leg of a middle-fized man; neverthelefs it may be convenient to have two other fizes, the one about twenty two inches in length, and the other fixteen.

TAB. V. Fig. I. A reprefents an under fplint of an irregular form, fuitable to that part of the leg it is intended to cover; it is a little convex externally, and concave internally. The length eighteen inches, from *a* to *b*. The width two inches and three quarters, at the ftrap near the knee, and two inches and a quarter at both the other ftraps.

B B B. Three leather ftraps from fifteen to twenty inches long, and one inch wide, having two rows of holes fo placed that every hole in each row may be oppofite to a fpace in the other. Thefe muft be fewed faft to the middle and outfide of the under fplint. The portions of ftraps *d d d* on the anteriour

Fig. 5.

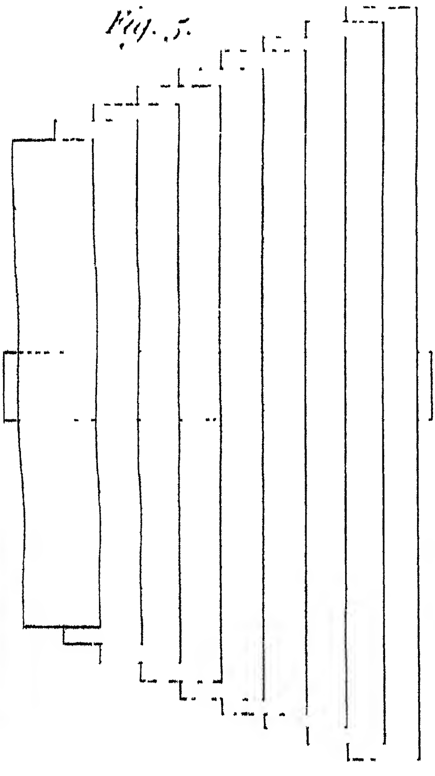


Fig. 4.

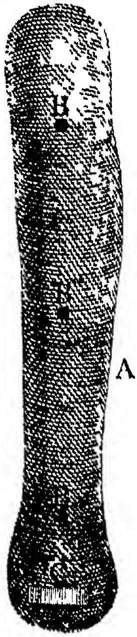
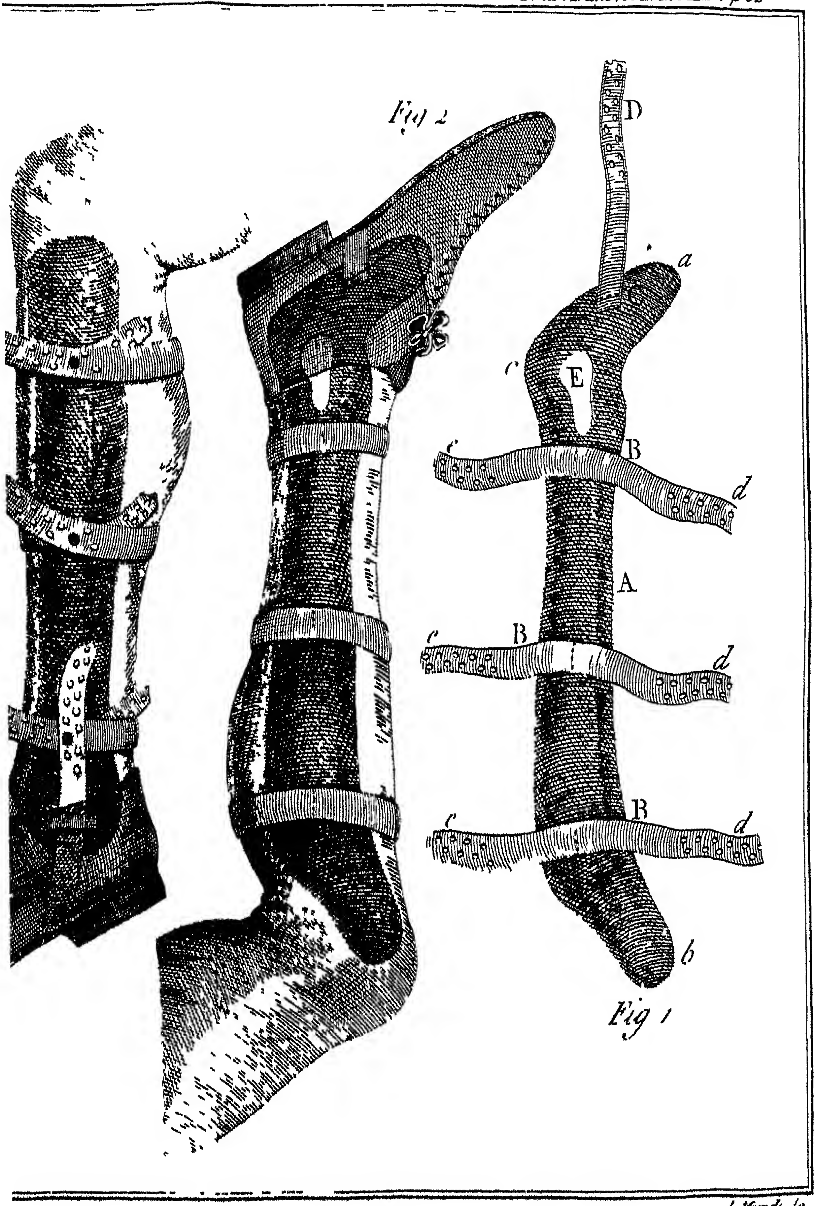


Fig. 3.



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part of the ſplint, muſt be ſhorter than thoſe on the poſteriour *e e e*, which are to ſurround the more muſcular part of the leg.

C. A part to ſupport the foot, from the point *a* to the heel *c*, five inches long, at an angle of ſixty degrees.

D. The foot-ſtrap, twelve inches long, ſewed to the bottom of the under ſplint, within two inches of the point, to paſs under the heel and through the leather loop on the upper ſplint, to the loweſt pin.

E. An irregular oval hole, two inches long and almoſt one wide in the loweſt part, but decreaſing upwards, to receive the *malleolus externus*, or lower extremity of the fibula.

Fig. II. Repreſents the leg raiſed up, to ſhew the ſituation of the under ſplint, when properly applied.

Fig. III. Repreſents a fractured leg, when laid within the ſplints, according to the method I am about to recommend, having the ſtocking-foot (or ſock) and ſhoe upon it: the darkeſt ſhade in this and Fig. II. being intended to ſhew that part of the ſplints within the ſhoe.

Fig. IV. A the upper ſplint. BBB the pins. C the leather loop to receive the foot-ſtrap.

Fig. V. A many-tailed bandage, made of ſlips of Ruſſia linnen, regularly increaſing in length from twelve or fourteen to eighteen or twenty inches, according to the ſize of the leg. Each of theſe ſlips (being two inches broad) is ſo laid as to cover half the breadth of that which is underneath it (viz. one inch). Another ſlip, ten or twelve inches long, is ſewed on the back ſo as to unite them all in the middle, making a bandage equally as firm as a circular one, and which

may be uſed without diſturbing the leg. The narroweſt part muſt be placed neareſt to the heel. As the number of ſlips are to be leſſened or increaſed according to the ſpace neceſſary to be incloſed within them, I call this a many-tailed bandage, leaving the preciſe number to be determined according to the nature of the circumſtances.

This has been uſed many years in St. Bartholomew's hoſpital, inſtead of the old eighteen-headed bandage, nevertheleſs, as it is not generally known, I hope this deſcription will not be thought ſuperfluous.

The three different ſizes of ſplints above-mentioned will generally be ſufficient; at leaſt one or other of them may ſerve any leg of an adult till others can be provided.

The legs of children, as they are more round and leſs muſcular, may be defended (nearly in the ſame manner) with the common wooden ſplints (properly bolſtered), that are now made uſe of in St. Bartholomew's hoſpital; provided they are long enough to ſecure both articulations of the fractured bone.

When a ſurgeon is called to a fractured leg, at the place where the accident happened, let him lay the patient on the injured ſide, upon a flat ſurface; and raiſe the knee of the fractured limb towards the abdomen, bending at the ſame time the knee joint, ſo as to put the extenſor muſcles of the foot (which are the ſtrongeſt) into a ſtate of relaxation. He will then be enabled to replace the ends of the fractured bones, and reſtore them to their proper ſituation, without the cuſtomary ſtrong extenſion of the limb; which is troubleſome to the ſurgeon, painful to the patient, and

apt

apt to bring on tenſion, ſpaſms, and inflammation of the ſtretched muſcles.

When the ends of the fractured bone are re-placed (which may often be done even without removing the ſhoe or ſtocking), let an under ſplint, of the moſt ſuitable ſize, be applied to the fibula, or outſide of the leg; and, if it does not fit exactly, let it be made to do ſo by adding ſuch compreſſes of tow, or thick flannel, as may be neceſſary for that purpoſe. Let alſo the upper ſplint be applied on the inſide of the leg, ſo as almoſt to cover the tibia, on its whole length. The ſtraps may then be faſtened upon the pins ſufficiently tight to ſecure the whole. This done, the patient may eaſily be removed in a ſedan chair (having the cuſhion ſo raiſed that the leg may hang down without reſting upon the bottom); or in a coach, with the limb ſupported by the hand of a ſurgeon, ſo that it may yield to every motion uniformly, or altogether as it were, whilſt it ſwings in his hand: for it matters not how great the motion of the body be, provided the points of the fractured bone are ſecured from being moved the one againſt the other.

In this manner I have carried many patients from the place of the accident, over the London pavements, to their own homes at a great diſtance, without their having ſuffered any inconvenience from the motion, even where the fractures were compound.

When the patient is brought home and put to bed, (the bed having a mattreſs upon it), let the ſtocking be removed, and the proper remedies, with the many-tailed bandage, and the above ſplints, applied; obſerving the ſame directions as to poſture, which I have already given: *viz.* that the patient be laid on his

his ſide with the broken leg undermoſt, the knee bent, and the thigh drawn up; inſtead of laying him on his back with the injured limb extended.

This poſture is much more comfortable and convenient (rendering it leſs troubleſome for the patient to eaſe himſelf, or be moved by others) and removes the foot and toes out of the way of being hurt by the weight of the bed-cloaths, ſo as to make a fracture-box or cradle unneceſſary.

If the fracture be compound, the wound generally heals by the firſt intention; the great impediment (irritation) being prevented, of which I can produce many inſtances. Add to this, that the leg may be taken up with the whole apparatus, and the knee joint gently moved, as often as neceſſary, to prevent that ſtiffneſs, which always ſucceeds a caſe of this kind, and is attended with much pain and inconvenience, a long time after the leg is, otherwiſe, well. The patient may alſo be taken out of bed frequently, without pain or danger, if not very heavy or unweildy.

The under ſplint makes a ſafe and ſecure bed for the leg, whilſt the upper part is dreſſing (if a compound fracture); and the leg may, by a ſteady hand, be ſupported alſo againſt the upper ſplint, and put into any poſture that may be neceſſary to dreſs a ſore on the fibula, or under part.

I do not always remove the ſhoe and ſtocking-foot; as well, becauſe they ſerve to keep the part in perſpiration, as, that the ſhoe adds ſteadineſs to the limb, by the connection it may have to the inferior part of the inſtrument. *Both the ſplints are, deſignedly, made narrow, to leave room for examination of the parts affected; leſt any undue preſſure ſhould*

occaſion

occasion pain: but, if it should be objected, that they are not sufficiently broad for a thick leg, that inconvenience may be remedied by putting a slip or pasteboard, or thin wood, between them on the fore-part of the leg, if thought necessary. The straps of the instrument are sufficient to secure the whole.

I made, with my own hand, the first of these instruments of strong pasteboard with iron plates rivetted upon them; which succeeded very well. I have tried also various materials for the same purpose, such as strong hide leather, hardened with glew; also wood, and plate-copper; any one of which will answer sufficiently, if well formed: nevertheless I shall prefer the pasteboard, if *made strong enough*, till I can meet with a workman, that will make them, accurately, of harder materials.

The same posture, that is recommended for broken legs, I have found equally serviceable with respect to broken thighs; and for the same reason: in which case the common wooden splint is as good as any other, provided it be long enough to secure both extremities of the fractured bone.

I have, through the whole of this description, made use of the terms upper and under splints, for the sake of being more easily understood: though perhaps the calling the one *tibiale*, and the other *fibularium*, would serve to distinguish them better, and give a more precise idea of the manner of their application; the first being placed so as to cover a great part of the tibia, and the second forming a safe bed for the fibula.

Received

Received October 20, 1766.

X. *Account of a locked Jaw, and Paralyfis, cured by Electricity: by Dr.* Edward Spry, *of* Totnefs, *in a Letter to* Charles Morton, *M. D. Sec. R. S.*

Read Feb. 19, 1767.

CATHARINE Smellidge, of Ditford, a girl aged eighteen, of a ftrong healthy conftitution, took, at the accidental death of a friend, a great fright, and the next day (Eafter-day, 1765) at his funeral, fell ill of very fevere convulfive fits, which lafted, with flight intermiffions, upwards of a month.

From the firft attack, fhe never fpoke, though otherwife fenfible; foon after her jaws became quite fixt, fo that fhe was obliged to be fed with thin panada, and the like, ftrained between her teeth, being not able to have them opened but a very little way, even by a wedge made for that purpofe. She became likewife paralytic from her hip down, on the right fide.

January 10, 1766. She confulted me, when I found her incapable of fupporting herfelf without affiftance, her leg and thigh of the right fide very torpid with a lofs of motion, and much more flaccid than the other, though not emaciated. She was incapable of uttering the leaft articulate found, or even of having her teeth fo far feparated by the

ſpeculum oris, as to admit my little finger between them.

The *maſſeter* and *temporal* muſcles, from their contraction, felt vaſtly tenſe, and rigid, being particularly painful on our preſſure thereon, or endeavour to open her mouth; the *genio-hyoidei* muſcles appeared alike circumſtanced, and the *platyſma-myoides* on the right ſide very often greatly convulſed.

Matters thus circumſtanced, after every uſual method judiciouſly adminiſtered by Mr. Guddrige of Brent, her ſurgeon, to little avail, I had but ſmall hopes from medicine; therefore recommended electricity; on which account, ſhe, having no opportunity of its being done in the country, came to her lodgings, taken in town for that purpoſe, on January 15, when, ſhe being ſomewhat inclined to be plethoric, and her menſes not hitherto interrupted, I ordered fourteen ounces of blood to be taken off, and the next day gave her a few ſlight (the feathered gnomon riſing not above the horizontal) electrical ſhocks on the leg of the diſeaſed ſide; ſhe immediately felt an agreeable ſenſation therein.

This proceſs was daily repeated, with a gradual increaſe of the *vis electrica*, ſometimes *plus*, ſometimes *minus*, electrifying her for ſix or ſeven days, by which time ſhe became much ſtronger, and capable of walking alone tolerably well.

I now (ſhe being, as to her jaw, and ſpeech, as at firſt) ſeveral times full-charged her with the electric matter, diſcharging it alternately from the *maſſeters*, her temples, and under the chin; immediately on her parting with which, ſhe, involuntarily, ſhook her

 head,

head, making her uſual noiſe, in endeavouring to ſpeak.

The next day, I fixed the conductor round her temples, and throat, and gave ſlight ſhocks, by touching ſometimes her chin, othertimes her teeth or cheeks, with the communicant wire. This ſhe diſagreeably, though advantageouſly, felt, her jaws hereby admitting their being opened a little.

The next day, I (the gnomon being near erect) increaſed the ſhocks conſiderably, by which, though ſhe very diſcontentedly bore them, ſhe became capable of opening her mouth to the width of an inch, and of articulating an imperfect, though, with difficulty, an intelligible ſound.

The next day, (the index quite perpendicular) ſhe very reluctantly received ſeveral ſmart ſhocks, and at laſt unexpectedly (the air being very electric) to ſuch a degree, as to deprive her of her ſenſes; ſhe becoming thereon, and remaining for half an hour, ſtrongly convulſed.

The next day, after the firſt ſhock, ſhe ſpoke ſo as to be tolerably well underſtood, telling us that the ſhocks were frequently vaſtly ſevere for her to bear; but that, as ſhe was fully ſenſible of the advantage ſhe had already received thereby, ſhe would gladly ſubmit to my will, in hopes of a further advantage.

She was even now incapable of bringing her tongue without her teeth, and of moving it without great difficulty, complaining it ſeemed very large, and heavy.

On inſpecting her mouth, which ſhe was able to open to almoſt its uſual width, I diſcovered nothing particular,

particular, but an extraordinary turgefcence, without induration, of the fublingual glands.

After this fhe received about twenty fhocks daily on her tongue, and other parts, for a fortnight, by which time all her complaints were removed, and fhe returned home quite well, and has remained fo ever fince,

N. B. In the firft week's experiments, the fhocks were confined between her hip, and foot, of the right fide; after that, on various parts, as judged requifite: her tongue, at its tip, became very red, and tender, after the firft electrization, its *papillæ* appearing very prominent; and its fubjacent glands foon leffened their bulk, her mouth running greatly with faliva: her pulfe, with a fhock or two, generally quickened twelve or fourteen times per minute. She, after grown tolerably well, immediately on having a fmart electrical ftroke, frequently became, for fome fmall time, as paralytic as ever on her right fide; and fometimes, thereon, had a return of her fits, the going off of which were attended with profufe fweats. Her blood appeared of a good texture, otherwife than giving off a little more than its due proportion of latex.

Received December 11, 1766.

XI. *Experiments on* Rathbone-Place *Water: By the Hon.* Henry Cavendifh, *F. R. S.*

Read Feb. 19, 1767.

DR. Lucas has given a fhort examination of this water in the firft part of his treatife of waters.* It is the produce of a large fpring at the end of Rathbone-place, and ufed a few years ago to be raifed by an engine for fupplying part of the town. The engine is now deftroyed; but there is a pump, nearly in the fame fituation, which yields the fame kind of water. It is the water of this pump, which was ufed in thefe experiments.

Moft waters, though ever fo tranfparent, contain fome calcareous earth, which is feparated from them by boiling, and which feems to be diffolved in them without being neutralized by any acid, and may therefore not improperly be called their unneutralized earth. The following experiments were made chiefly with a view of enquiring into the caufe of the fufpenfion of this earth, for which purpofe this water feemed well adapted; as it contains more unneutralized earth than moft others.

Thefe experiments were made towards the latter end of September 1765, after a very dry fummer; whereby the water was moft likely more impregnated with faline and other matters than it ufually is.

The

The water, at the time I uſed it, looked rather foul to the eye. On expoſing ſome of it for a few days to the open air, a ſcurf was formed on its ſurface, which was nothing elſe but ſome of the unneutralized earth ſeparated from the water. On dropping into it a ſolution of corroſive ſublimate, it grew cloudy in a few ſeconds; it quickly became opake, and let fall a ſediment. This is a property, which I believe does not take place, in any conſiderable degree, in moſt of the London waters.

EXPERIMENT I.

494 ounces of this water were diſtilled in a copper ſtill, till about 150 oz. were drawn off. A good deal of earth was precipitated during the diſtillation, which being collected and dried, weighed 271 grains. It proved to be entirely a calcareous earth, except a ſmall part, which was magneſia. This I found in the following manner. A little of this earth, being mixed with ſpirit of ſalt, diſſolved entirely; which ſhews it to conſiſt ſolely of an abſorbent earth, but does not ſhew whether it is a calcareous earth or magneſia. The remainder was ſaturated with oil of vitriol: a great deal of matter remained undiſſolved, which, as the earth was ſhewn to be entirely of the abſorbent kind, muſt have been ſelenite, or a calcarious earth ſaturated with the oil of vitriol. The clear liquor ſtrained from off the ſelenite yielded on evaporation only eighteen grains of ſolid matter, which proved to be Epſom ſalt; ſo that all the earth, except that contained in the eighteen grains of Epſom ſalt, muſt have been of the calcareous kind. That contained in the Epſom ſalt is well known to be magneſia.

The

The water remaining after diſtillation, and from which the earth was ſeparated, was evaporated, firſt in a ſilver pan, and afterwards in a glaſs cup, till it was reduced to about three ounces. Not the leaſt earth was precipitated during the evaporation, till it was reduced to a ſmall quantity; there then fell 39 grains, which were entirely ſelenite: ſo that all the unneutralized earth in the water was ſeparated during the diſtillation. The liquor thus evaporated was of a reddiſh colour, like an infuſion of ſoot.

Many waters contain a good deal of neutral ſalt compoſed of the nitrous acid united to a calcareous earth; the moſt convenient way of aſcertaining the quantity of which, is to drop a ſolution of fixed alcali into the evaporated water, till all the earth is precipitated; whereby this ſalt is changed into true nitre, and is capable of being cryſtallized. For this reaſon, ſome fixed alcali was dropt into the evaporated water till it made no farther precipitation. The earth precipitated thereby weighed thirty-ſix grains, and was entirely magneſia. The liquor was then farther evaporated, but no nitre could be made to ſhoot: being then evaporated to dryneſs, it weighed 256 grains. It gave not the leaſt ſigns of containing any nitrous ſalt, either by putting ſome of it upon lighted charcoal, or by making a match with a ſolution of it, but appeared to be a mixture of ſea ſalt and vitriolated tartar, or ſome other ſalt compoſed of the vitriolic acid. As I have heard of no other London water, that has been examined with this view, but what has been found to contain a conſiderable proportion of nitrous ſalt, it ſeems very remarkable that this ſhould be intirely deſtitute of it. I now

proceed to the experiments made on the diſtilled water.

The diſtilled water, eſpecially that part of it which came over firſt, became opake, and let fall a precipitate, on droping into it a ſolution of ſugar of lead. It alſo became opake by the addition of corroſive ſublimate, much in the ſame manner that the plain water did before diſtillation.

It was found, by dropping into it a little acid of vitriol and committing it to evaporation, to contain a ſmall quantity of volatile alcali; as it left four grains of a browniſh ſalt, which being re-diſſolved in water, yielded a ſmell of volatile alcali on the addition of lime. It is doubtleſs this volatile alcali, which is the cauſe of the precipitate, which the diſtilled water makes with ſugar of lead and corroſive ſublimate.

What firſt ſuggeſted to me that the diſtilled water contained a volatile alcali, was the diſtilling ſome of it over again in a retort; whereby the firſt runnings were ſo much impregnated with volatile alcali, as to turn paper died with the juice of blue flowers, to a green colour, and in ſome meaſure to yield a ſmell of volatile alcali.

In the foregoing experiment, the ſalt procured from the diſtilled water was perfectly neutral; ſo that the quantity of acid employed was certainly not more than ſufficient to ſaturate the alcali, but it may very likely have been leſs; as in that caſe the ſuperfluous volatile alcali would have flown off in the evaporation. The following experiment ſhews pretty nearly the quantity of volatile alcali in the diſtilled water.

EXPERI-

Experiment II.

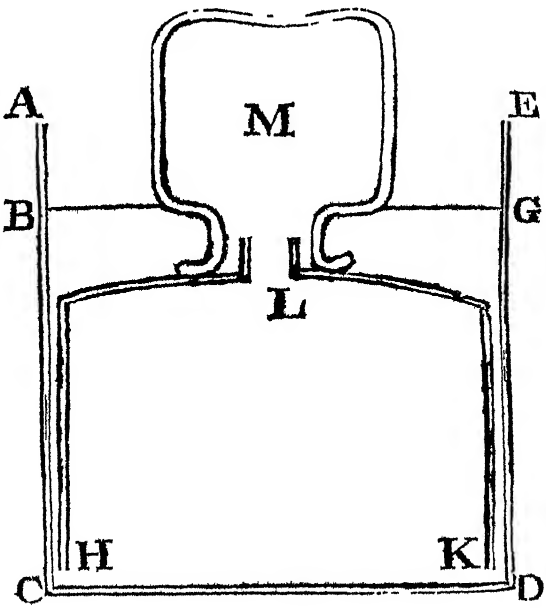
1128 ounces of Rathbone place water were diſtilled in the ſame manner as the former. The diſtilled water was divided into two parcels, that parcel which came over firſt weighing 121 ounces, the other 146. A preparatory experiment was firſt made, in order to form a judgement of the comparative ſtrength of each parcel, and alſo of the quantity of acid which it would require to ſaturate them. This was done by dropping ſugar of lead into each parcel till it ceaſed to make a precipitate. It was judged from hence that the firſt parcel contained about $2\frac{1}{2}$ times as much volatile alcali as an equal quantity of the ſecond. Into 30 ounces of the firſt parcel, mixed with as much of the ſecond, was then put 43 grains of oil of vitriol, which was ſuppoſed to be about $\frac{1}{4}$ more than ſufficient to ſaturate the alcali therein. The mixture was then evaporated. When reduced to a ſmall quantity, it was found to be rather acid: ſixteen grains of volatile ſal ammoniac were therefore added, which ſeemed nearly ſufficient to neutralize it. Being then evaporated to dryneſs, it left ſixty-ſix grains of a browniſh ſalt, which diſſolved readily in water, leaving only a trifling quantity of brown ſediment. A little of this ſalt was found to make no precipitate on the addition of fixed alcali, and the remainder, being boiled with lime, was converted into ſelenite; a ſure ſign that the ſalt was merely vitriolic ammoniacal ſalt. The volatile alcaline ſalt contained in ſixty ſix grains of vitriolic ammoniacal ſalt is $58\frac{1}{2}$ grains; from whence deducting ſixteen grains, the weight of the volatile ſal ammoniac added, it appears that the diſtilled

water

water uſed in this experiment contains 42½ grains of volatile ſalt; and therefore the whole quantity of volatile ſalt driven over by diſtillation ſeems to be about ſixty-eight grains, which, as the ſecond parcel was ſo much weaker than the firſt, is probably nearly the whole volatile alkali contained in the water.

Experiment III.

Dr. Brownrig, in a paper printed in the Philoſophical Tranſactions, for the year 1765, ſhews that a great deal of fixed air is contained in Spa water. This induced me to try whether I could not find any in that of Rathbone-place; which I did by means of the contrivance repreſented in the drawing.



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ACDE reprefents a tin pan, filled with Rathbone-place water as high as BG. HKL is another tin pan, within the firft, in the manner of an inverted funnel, and made in fuch a manner as to leave as little room as poffible between that and the fides of the outward veffel. M reprefents a bottle, full of the fame water, inverted over the mouth of the funnel. By this means, as faft as the air is difengaged by heat from the water within the funnel, it muft neceffarily rife up into the bottle. The Rathbone-place water, put into the veffel, weighed 411 ounces, the funnel held 353 ounces. A bottle full of water being inverted over the mouth of the funnel, as in the figure, the water was heated, and kept boiling about $\frac{1}{4}$ of an hour. As foon as one bottle was filled with air, it was removed by putting a fmall ladle under its mouth, while under water, and fet with its mouth immerfed in the fame manner in another veffel of water, taking care not to fuffer any communication between the included air and the outward air during the removal. At the fame time, another bottle full of water was inverted over the mouth of the funnel, in the fame manner as the former. It was not eafy telling how much air was difcharged from the water; as the air in the bottles, when firft removed, was hot and expanded; and, before I could be fure it was cold, there was fome of it abforbed by the water: but there feemed to be above 75 ounce meafures difcharged, fcarce twenty of which arofe before the water began to boil. The water continued difcharging air after the experiment was difcontinued. In about a day's time, much the greateft part of the air was abforbed, fcarce fixteen u nce meafures remaining. That which was abforbed appeared to be fixed air, as the water which had abforbe d

abſorbed it made a precipitate with lime-water. But, in order to abſorb all the fixed air more perfectly, the air which remained not abſorbed was transferred into another bottle of water, in the manner deſcribed in my firſt paper on factitious air, page 142 of the preceding volume. This bottle was then ſet with its mouth immerſed in a bottle of ſope-leys; after which, by ſhaking the bottle, the ſope-leys was mixed with the included water; whereby the air in the bottle was brought in contact with the ſope-leys, which is well known to abſorb fixed air very readily. By this means the air was reduced to 8$\frac{3}{4}$ ounce meaſures. A ſmall vial being filled with equal quantities of this and inflammable air, and a piece of lighted paper applied to its mouth, it went off with as loud a bounce, as when the ſame vial was filled with equal quantities of common air and inflammable air. The ſpecific gravity of the remainder was tried by a bladder, in the manner deſcribed in the above-mentioned paper; as well as could be judged from ſo ſmall a quantity, it was juſt the ſame as that of common air. From theſe two circumſtances, I think we may fairly conclude that this unabſorbed part was intirely common air; conſequently the air diſcharged from the Rathbone-place water conſiſted of 8$\frac{3}{4}$ ounces of common air and about 66 of fixed air. The air which was diſcharged before the water began to boil contained much more common air, than that which was diſcharged afterwards; that which was diſcharged towards the latter end ſeeming to contain ſcarce any but fixed air.

As ſo much fixed air is diſcharged from this water by boiling, it ſeemed reaſonable to ſuppoſe, that the diſtilled water ſhould contain fixed air. I accordingly found it to make a precipitate with lime-water.

 EXPE-

EXPERIMENT IV.

The following experiment ſhews that the fixed air was not generated during the boiling, but was contained in the water before. Into 30 ounces of Rathbone-place water was poured ſome lime-water, which immediately made a precipitate. More lime-water was added, till it ceaſed to make any farther precipitate. It required 20¼ ounces. The precipitated earth being dried weighed 39 grains.

The unneutralized earth contained in 30 ounces of Rathbone-place water is 16½ grains, and the earth contained in 20¼ ounces of lime-water (as was found by precipitating the earth by volatile ſal ammoniac) is 21 grains. Therefore the earth precipitated from the mixture of Rathbone-place water, and lime-water, is about equal to the ſum of the weights, of the earth contained in the lime-water, and of the unneutralized earth in the Rathbone-place water; and conſequently all the unneutralized earth ſeems to be precipitated from Rathbone-place water by the addition of a proper quantity of lime-water. But a more convincing proof that this is the caſe, is that the clear liquor, after the precipitate had ſubſided, did not depoſit any earth on boiling, or become in the leaſt cloudy on the addition of fixed alkali; whereas Rathbone-place water in its natural ſtate becomes opake thereby. It might perhaps be expected, that the clear liquor ſhould ſtill make a precipitate on the addition of fixed alkali, though the unneutralized earth is precipitated; as in all probability there is ſtill a good deal of earth remaining in it in a neutralized ſtate. The reaſon why it does not, ſeems to be, that the remaining earth is moſt likely intirely magneſia; and Epſom ſalt,

ſalt, when diſſolved in a great quantity of water, does not make any precipitate on the addition of fixed alkali.

There is great reaſon to ſuppoſe that the earth precipitated on mixing the Rathbone-place water and lime-water, was very nearly ſaturated with fixed air, i. e. that it contained very near as much fixed air, as is naturally contained in the ſame quantity of calcarious earth. If ſo, 30 ounces of Rathbone-place water contain as much fixed air as 39 grains of calcarious earth; whereas the unneutralized earth, in that quantity of water, is only 16 $\frac{1}{2}$ grains; ſo that Rathbone place water contains near 2 $\frac{1}{2}$ times as much fixed air as is ſufficient to ſaturate the unneutralized earth in it.

It ſeems likely from hence, that the ſuſpenſion of the earth in the Rathbone-place water, is owing merely to its being united to more than its natural proportion of fixed air; as we have ſhewn that this earth is actually united to more than double its natural proportion of fixed air, and alſo that it is immediately precipitated, either by driving off the ſuperfluous fixed air by heat, or abſorbing it by the addition of a proper quantity of lime water.

Calcareous earths, in their natural ſtate, i. e. ſaturated with fixed air, are totally inſoluble in water; but the ſame earths, entirely deprived of their fixed air, i. e. converted into lime, are in ſome meaſure ſoluble in it; for lime-water is nothing more than a ſolution of a ſmall quantity of lime in water. It is very remarkable, therefore, that calcareous earths ſhould alſo be rendered ſoluble in water, by furniſhing them with more than their natural proportion of fixed air, i. e. that they ſhould be rendered ſoluble, both by depriving them of their fixed air,

and

and by furniſhing them with more than their natural quantity of it. Yet, ſtrange as this may appear, the following experiments, I think, ſhew plainly that it is the real caſe.

Experiment V.

In order to ſee whether I could ſuſpend a calcareous earth in water, by furniſhing it with more than its natural proportion of fixed air, I took 30 ounces of rain water, and divided it into two parts: into one part I put as much ſpirit of ſalt, as would diſſolve 30 $\frac{3}{10}$ grains of calcareous earth, and as much of a ſaturated ſolution of chalk, in ſpirit of ſalt, as contained 20 grains of calcaroeus earth: into the other part I put as much fixed alkali, as was equivalent to 46 $\frac{8}{10}$ grains of calcareous earth, i. e. which would ſaturate as much acid. This alkali was known to contain as much fixed air as 39 grains of calcareous earth. The whole was then mixed together and the bottle immediately ſtopped. The alkali was before ſaid to be equivalent to 46 $\frac{8}{10}$ grains of calcareous earth, and was, therefore, ſufficient to ſaturate all the ſpirit of ſalt, and alſo to decompound as much of the ſolution of chalk as contains 16 $\frac{1}{2}$ grains of earth. This mixture, therefore, ſuppoſing I made no miſtake in my calculation, contained 16 $\frac{1}{2}$ grains of unneutralized earth, with as much fixed air as is contained in 39 grains of calcareous earth; which is the quantity which was found to be in the ſame quantity of Rathbone place water. The mixture became turbid on firſt mixing, but the earth was quickly re-diſſolved on ſhaking, ſo that the liquor became almoſt tranſparent. After ſtanding ſome time, a ſlight ſediment fell to the bottom, leaving the liquor perfectly tranſparent.

tranſparent. The mixture was kept three or four days ſtopped up, during which time it remained perfectly clear, without depoſiting any more ſediment. The clear liquor was then poured off from the ſediment, and boiled for a few minutes, in a Florence flaſk; it grew turbid before it began to boil, and diſcharged a good deal of air; ſome earth was precipitated during boiling, which being dried weighed 13 grains.

This ſhews that there was really, at leaſt 13 grains of earth ſuſpended in this mixture, without being neutralized by any acid; the ſuſpenſion of which could be owing only to its being united to more than its natural proportion of fixed air. But, as a further proof of this, I made the following experiment.

Experiment VI.

I took the ſame quantities of rain water, ſolution of chalk, ſpirit of ſalt, and fixed alkali, as in the laſt experiment, but mixed them in a different order. The fixed alkali was firſt dropped into the ſpirit of ſalt, and when the efferveſcence was over, was diluted with $\frac{1}{2}$ the rain water. The ſolution of chalk was then diluted with the remainder of the rain water, the whole mixed together, and the bottle immediately ſtopped, and ſhook vehemently. A precipitate was immediately formed on mixing, which could not be re-diſſolved on ſhaking.

It muſt be obſerved, that, in the firſt of the two foregoing experiments, all the fixed air contained in the alkali was retained in the mixture, none being loſt by efferveſcence; whereas, in the laſt experiment, the greateſt part of the fixed air was diſſipated in the efferveſcence; no more being retained than

what

what was contained in that portion of the fixed alkali, which was not neutralized by the acid; and consequently the unneutralized earth, in the mixture, contained not much more fixed air than what was sufficient to saturate it. As the latter of these mixtures differed no otherwise from the former, than that it contained less fixed air; the suspension of the earth in the former must necessarily be owing to the fixed air.

In the two foregoing experiments the water contained, besides the unneutralized earth, and fixed air, some sal sylvii, and a little solution of chalk in the marine acid; which, it may be supposed, contributed to the suspension of the earth: but the following experiment shews that a calcarious earth may be suspended in water, without the addition of any other substance than fixed air.

Experiment VII.

A bottle full of rain water was inverted into a vessel of rain water, and some fixed air forced up into the bottle, at different times, till the water had absorbed as much fixed air as it would readily do; 11 ounces of this water were mixed with $6\frac{1}{2}$ of lime water. The mixture became turbid on first mixing, but quickly recovered its transparency, on shaking, and has remained so for upwards for a year.

This mixture contains 7 grains of calcareous earth; and, from a subsequent experiment, I guess it to contain as much fixed air, as there is in 14 grains of calcareous earth.

Experiment VIII.

Least it should be supposed, that the reason why the earth was not precipitated in the foregoing experi-

ment, was, that it was not furniſhed with a ſufficient quantity of fixed air, the following mixture was made, which contains the ſame proportion of earth as the former, but a leſs proportion of fixed air: $4\frac{3}{4}$ ounces of the above-mentioned water, containing fixed air, were diluted with $6\frac{1}{4}$ of rain water, and then mixed with $6\frac{1}{2}$ ounces of limewater. A precipitate was immediately made on mixing, which could not be re-diſſolved on ſhaking.

Experiment IX.

I made ſome experiments to find whether the unneutralized earth could be precipitated from other London waters, by the addition of lime water, as well as from Rathbone-place water. It is neceſſary for this purpoſe, that the quantity of lime water ſhould be adjuſted very exactly; for, if it is too little, it does not precipitate all the unneutralized earth; if it is too great, ſome of the earth in the lime water remains ſuſpended. For this reaſon, as I found it almoſt impoſſible to adjuſt the quantity with ſufficient exactneſs, I added ſuch a quantity of lime water, as I was well aſſured, was more than ſufficient to precipitate the whole of the unneutralized earth; and when the precipitate was ſubſided, decanted off the clear liquor, and expoſed it to the open air, till all the lime remaining in the water was precipitated, by attracting fixed air from the atmoſphere. The clear liquor was then decanted and evaporated, which is much the moſt exact way I know of ſeeing whether any unneutralized earth remains ſuſpended in the water. The reſult of the experiments was as follows:

200 ounces of water, from a pump in Marlborough-ſtreet, were mixed with 38 ounces of lime water. The earth precipitated thereby weighed 38 grains. The clear liquor, expoſed to the air, and evaporated in a ſilver pan till it was reduced to 6 or 7 ounces, depoſited no more than 2 or 3 grains of unneutralized earth.

A like quantity of the ſame pump water, evaporated by itſelf without the addition of lime water, depoſited about 19 grains of unneutralized earth.

200 ounces of water, from a pump in Hanover-ſquare, being mixed with 67 ounces of lime water, the precipitate weighed 93 grains. The clear liquor, treated in the ſame way as the former, depoſited about 2 grains of earth. 200 ounces of the ſame water, evaporated by itſelf, depoſited 28 grains of earth.

The ſame quantity of water from a pump in St. Martin's church-yard, being mixed with 82 ounces of lime water, the precipitate weighed 108 grains. The clear liquor depoſited ſcarce any unneutralized earth on evaporation.

The ſame quantity of water, evaporated by itſelf, yielded 45 grains of unneutralized earth.

The way, by which I found the quantity of unneutralized earth depoſited on evaporation, was, after having decanted the clear liquor, and waſhed the reſiduum with rain water, to pour a little ſpirit of ſalt into the ſilver pan, which diſſolves all the calcareous earth, but does not corrode the ſilver. Then, having ſeparated the ſolution from the inſoluble matter, the earth was precipitated by fixed alcali.

In this way of finding the quantity of unneutralized earth, care muſt be taken to add very little more

acid

acid than is neceſſary to diſſolve the unneutralized earth, and to uſe as little water in waſhing out the ſolution as poſſible; for otherwiſe a good deal of the ſelenite, which is depoſited in the evaporation of moſt water, will be diſſolved; the earth of which will be precipitated by the fixed alcali, and by that means make the quantity of unneutralized earth appear greater than it really is.

It appears from theſe experiments, that the unneutralized earth is intirely precipitated from theſe three waters, by the addition of a proper quantity of lime water; as the trifling quantity found to be depoſited, on the evaporation of two of them, moſt likely proceeded only from not expoſing the water to the air, long enough for all the lime to be precipitated. So that I think it ſeems reaſonable to conclude, that the unneutralized earth, in all waters, is ſuſpended merely by being united to more than its natural proportion of fixed air.

To return to Rathbone-place water; it appears from the foregoing experiments, that one pint of it, or 7315 grains, contains, firſt, as much volatile alcali as is equivalent to about $\frac{9}{10}$ grains of volatile ſal ammoniac: ſecondly, 8 $\frac{4}{10}$ grains of unneutralized earth, a very ſmall part of which is magneſia, the reſt a calcareous earth: thirdly, as much fixed air, including that in the unneutralized earth, as is contained in 19 $\frac{8}{10}$ grains of calcareous earth: fourthly, 1 $\frac{2}{10}$ of ſelenite: fifthly, 7 $\frac{9}{10}$ of a mixture of ſea ſalt, and Epſom ſalt; and the whole ſolid contents of 1 pint of the water is 17 $\frac{1}{2}$ grains.

One pint of water, from the pump in Marlborough-ſtreet, contains 1 $\frac{4}{10}$ grains of unneutralized

earth, and as much fixed air as is contained in 2 $\frac{[illegible]}{10}$ grains of calcareous earth.

The ſame quantity of water, from the pump in Hanover-ſquare, contains $2\frac{1}{10}$ grains of unneutralized earth, with as much fixed air as is contained in $7\frac{2}{10}$ of earth.

The ſame quantity of water, from St. Martin's Church-yard, contains $3\frac{4}{10}$ grains of unneutralized earth, with as much fixed air as is contained in $8\frac{2}{10}$ of earth.

Received November 18, 1766.

XII. *Deſcription of a Meteor ſeen at* Oxford, October 12, 1766. *In a Letter to* Charles Morton, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Dear Sir,

Read Feb. 26, 1767.

THE Reverend Dr. Sharp coming into Chriſt-Church common-room out of the great quadrangle, on Saturday, October 12, 1765, about 8^h $30'$ P. M. informed the company there, that he had ſeen ſome remarkable *Auroræ Boreales* a few minutes before. But, as ſuch phænomena

[illegible]

[illegible]

mena are common enough here, they gave little attention to the information. However, being upon the terrace, about 8h 45′ P. M. I diſcovered (See Tab. V. *) a broad luminous arch, in the northern part of the hemiſphere, extending from E. to W. almoſt terminated by the horizon, and ſomewhat leſs than a ſemicircle. The upper or exterior limb of this arch, together with a certain portion of the lucid adjoining tract, was white and reſplendent; but the brightneſs gradually decreaſed as it approached the lower or interior limb, which was ſo fuſcous and obſcure, that it ſeemed ſcarce diſtinguiſhable from the clouds that were contiguous to it. For about five minutes, the luſtre remained pretty ſtrong and vivid, and the meteor without any viſible change or variation; but, after the expiration of that ſhort term, the arch began to grow faint, and in one or two minutes more, as near as I can gueſs, totally diſappeared.

How long this meteor had been formed, when I firſt obſerved it, I cannot take upon me to ſay; but I believe it was then, and perhaps for ſome time had been, upon the decline. It was ſeen by the Reverend Mr. Selſtone and the Reverend Mr. Beſt, chaplains of Chriſt-Church, as well as by other members of the Univerſity, not without ſome degree of wonder and ſurprize. The crepuſculum, or illuſtration of the atmoſphere, which ſometimes precedes ſuch meteors as that deſcribed here, and even continues long after their extinction, might perhaps have remained till ten or eleven o'clock; which if we admit, this crepuſculum may not improbably be conſidered as the ſame phænomenon with "the ſurprizing "bright * luminous appearance viſible at London in

* LLOYD's *Evening Poſt*, &c. No. 1289. p. 365.

the

" the hemisphere from the East to the West, about " ten o'clock, which lasted about an hour," the same night, or at least as something similar to it. In either of which cases, the atmosphere at London will be allowed to have been in a proper disposition for the production of this species of meteors, and impregnated with the same kind of luminous vapour that occasioned the phænomenon mentioned here.

The singularity of this meteor was fixed by the gradual and regular diminution of its resplendency between the upper and lower limbs, an instance of which I never observed before. This continued from the time I first discovered the arch almost to the very moment of its extinction. The limbs of the zone forming this arch were, however, very well defined; insomuch that the regularity of its figure, by the gradual decrease of brightness, was not in the least impaired. The evening was somewhat cloudy, but still and calm, and several of the stars appeared. As I have not met with a description of such a phænomenon, in any of the physiological papers consulted by me on this occasion; I have taken the liberty to transmit you the foregoing account of the meteor seen here, on Saturday, October 12, 1766, to be communicated to the Royal Society, hoping it may prove not altogether unacceptable to the members of that most learned and illustrious body. I am, with great regard,

Dear Sir,

Your most obliged

humble Servant,

Christ-Church, Oxon.
Nov. 7, 1766.

John Swinton.

Received

Received November 18, 1766.

XIII. *Some Obſervations on Swarms of Gnats, particularly one ſeen at* Oxford, Auguſt 20, 1766. *In a Letter to* Charles Morton, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Dear Sir,

Read Feb. 26, 1767.

THE gnats have been more numerous, as well as more noxious, here, during the months of July, Auguſt, and September, 1766, than perhaps they were ever known to be before in the memory of man. So many myriads of them have ſometimes occupied the ſame part of the atmoſphere, in contiguous bodies, that they have reſembled a very black cloud, greatly darkened the air, and almoſt totally intercepted the ſolar rays. The repeated bites likewiſe of theſe malignant inſects have been ſo ſevere, that the legs, arms, heads, and other parts, affected by them, in many perſons, have been ſwelled to an enormous ſize. The colour alſo of theſe parts, at the ſame time, was red and fiery, perfectly ſimilar to that of ſome of the moſt alarming inflammations.

As the natural hiſtory of theſe troubleſome inſects is ſufficiently known, having been given us, with great

accuracy and precision, by Moufet, Swammerdam, Reaumur, and others, it would be superfluous to expatiate upon them here. But I cannot forbear taking notice of one very remarkable property of these little mischievous animals, which lately presented itself to my view, and which has not yet perhaps been duly attended to by any naturalist. Being in the garden belonging to the Fellows of Wadham College, with the Reverend Mr. Allen, Fellow of that house, on Wednesday, August 20, 1766, about half an hour before sun-set, such an immense number of gnats filled the atmosphere, in which we breathed, as I had never seen before.. We both of us also then observed six columns, formed intirely of these insects, ascending from the tops of six boughs of an apple-tree, in another garden, separated from that we were in by a partition-wall, to the height of at least fifty or sixty feet. Two of these columns seemed perfectly erect and perpendicular, three of them oblique, and one approached somewhat towards a pyramidal form. That bodies of gnats, in figure a little similar to the pillars mentioned here, are now and then to be seen, we learn from an ingenious * author; but that these bodies ever ascend fifty or sixty feet, has not, I believe, been yet observed by any zoologer or natural historian.

It may not be improper to remark, that some of these gnats had their bodies greatly distended, and swoln much beyond their usual size, by the uncommon quantities of blood they had imbibed. One of them, in particular, being killed at the castle here, seemed considerably larger than any of the rest, and

* Tho. Moufet, *Insector. sive Minimor. Animal. Theatr.* c. xiii. p. 82. Londini, 1634.

had as much blood expressed from it as stained or besmeared part of a wall three or four inches square.

I have been informed by the Reverend Dr. Wyndham, Warden of Wadham College in this University, that, about thirty years ago, many columns of gnats were perceived to rise from the top of the steeple of the cathedral church at Salisbury, by a considerable number of people. He likewise declared, that these columns were seen both by himself and the Reverend Dr. John Clarke, then Dean of Salisbury; that, at a small distance, they resembled smoke; and that this at first occasioned a sort of alarm, many believing that the church was on fire.

I shall only beg leave at present to add, that the Reverend Mr. Berkeley, Student of Christ-Church, as he assured me, was entertained with an appearance similar to that I had seen, the same evening, in the country, at a small distance from Oxford; that some of the bodies of gnats perceived by him, unless he was deceived, ascended much higher than those I had observed; and that I am, with all due sentiments of respect, Sir,

Your much obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.
Nov. 15, 1766.

John Swinton.

XIV. *A Description of the Andrachne, with its Botanical Characters: By* G. D. Ehret, *F. R. S.*

Read Feb. 26, 1767.

FROM a short and crooked stem go off irregularly several branches bending in various directions; but the younger shoots mostly pointing upwards. The height of the shrub is now about four feet.

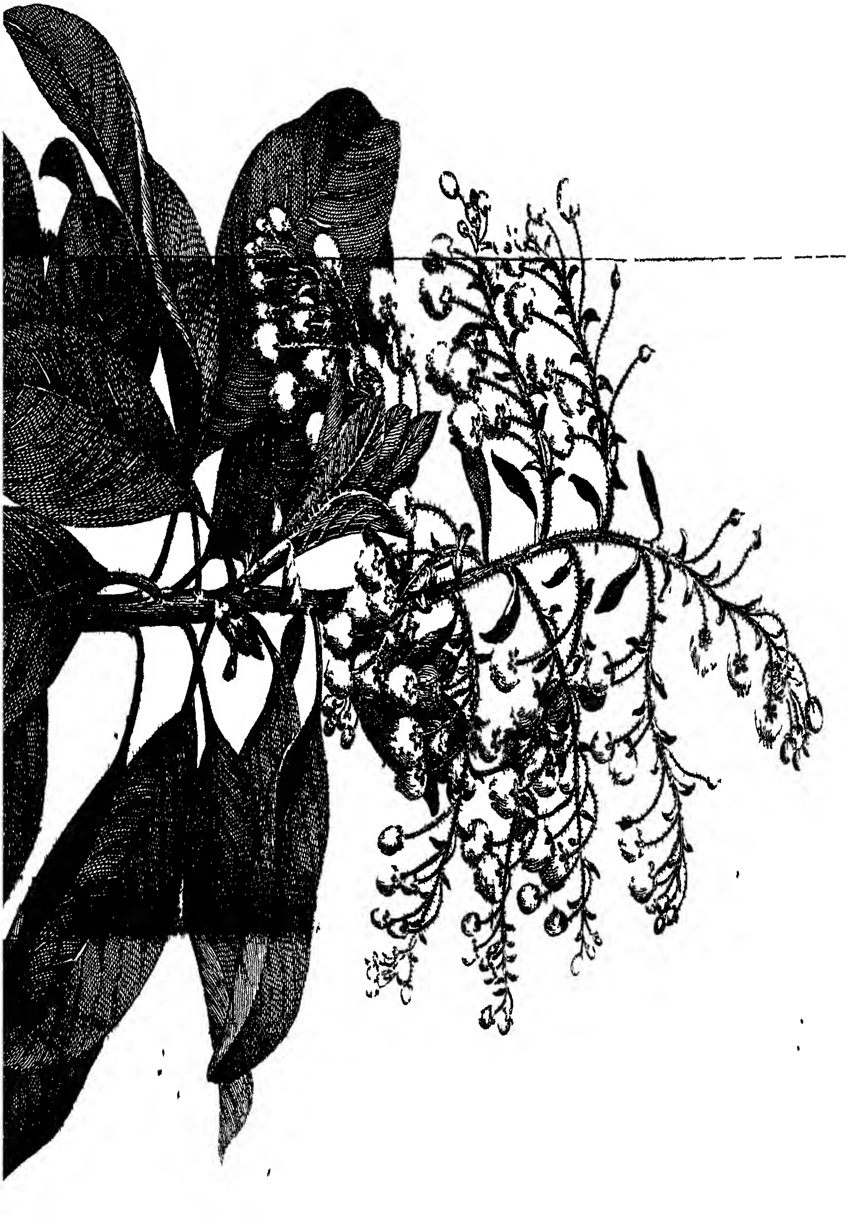
The stem and branches are of different colours at different seasons. In the spring, they appear of a greenish cinnamon colour; this is gradually heightened to almost a red during winter; towards the end of which, the epidermis peels off, and the new bark exhibits the like appearance as it had the spring before.

On the extremities of these branches, the shoots of the preceding year, which are of a deep red colour, are many leaves of different sizes, placed irregularly; the largest leaves were in length, when the figure was drawn, about four inches, and two inches and an half in breadth, of an oval figure: they are mostly entire, though the edges of some are lightly serrated: their surface is smooth and lively, but not glossy or shining. They are supported on the branches by footstalks about an inch long, of a red colour and smooth.

The young leaves, at their first appearence, are of a faintish green with a cast of yellow yet beautifully shaded with red: their footstalks and middle rib are

ARBUTUS (Andrachne) frutescens, Spica erecta, Foliis ovatis integris, et serratis, Bacca tuberculosa polysperma.

G. D. Ehret pinx. 1760



then hoary, but they loſe this appearance as they grow older.

This very rare ſhrub produced its flowers, for the firſt time in England, in the garden of Dr. John Fothergill, at Upton near Stafford in Eſſex, May 1766. The principal ſpikes of flowers in this ſpecies of arbutus are erect, producing many ſide ones in a horizontal direction, their extremities inclining downwards. Each of theſe ſimple ramifications contain many white globular flowers, hanging on long hoary glutinous ~~pedunculi, which are ſituated alternately.~~ Theſe ſpikes of flowers, forming a kind of looſe tuft with the bright bunches of leaves, form an elegant appearance.

Characters of the Flower.

TAB. VI.

Fig. *a.* repreſents a ſide view of the flowers; they are of a globular ſhape, and open into five obtuſe reflex laciniæ, in the manner of the common arbutus.

Fig. *b.* a back view of the flower, upon which appears the calyx ſpread open, and cloſely adhering to the flower; it conſiſts of five oval pointed leaves or diviſions; around this calyx appear on the corolla ten viſible nectaria.

When theſe flowers drop off, the calyx cloſes up, and embraces the tender germ. See Fig. *c.*

Fig. *d.* repreſents a flower ſeparated from the calyx; it is inſerted at the baſe of the germen. The ten nectaria, which are ſomewhat ſwollen or raiſed from the corolla, and have tranſparent appearances, are

alſo diſcoverable, whilſt the magnified figure *e* lays the parts more diſtinctly in view. This is a remarkable character in this flower.

Fig. *f*. exhibits the flower laid open: it is ſmooth without, and hoary within; it contains ten ſtamina, which are inſerted at the baſe of the flower, their filaments and apices embrace half the ſtyle.

Fig. *g*. two ſtamina magnified, the baſe whereof is a tender fleſhy ſubſtance, hoary and of a club-like ſhape; this diminiſhes gradually into a filament, upon which is ſituated a ſingular anthera; this anthera burſts at two apertures (as the figure repreſents), and diſperſes its farinaceous duſt towards the ſtyle: from the top of this apex, comes forth, at the oppoſite ſide, two crooked forked horns, bending downwards in length of the antera.

Fig. *h*. the germen or rudiment magnified. This is hoary, its baſe conſiſts of a red fleſhy ſubſtance, with ten obtuſe angles. The ſtyle ſupports a ſmall globular ſtigma, and does not exceed the length of the flower.

Fig. *i*. repreſents a horizontal view of the germen, as obſerved through a lens; it has five regular loculaments or cells, though ſeemingly but one ſeed; but by a cloſer inſpection, there appeared ſeveral embryo ſeeds in each cell.

Fig. *k*. a dried fruit or berry of the andrachne in its natural ſize, with an horizontal ſection. This fruit, which is tuberculous, I drew from a ſpecimen conſiſting of the whole branch, leaves, flower-ſpikes, with many ripe berries which was brought from Aleppo, by Dr. Alexander Ruſſell; all which I examined and deſcribed at that time for my own ſatisfaction, and find

find them to agree exactly with the recent ſhrub above deſcribed. It likewiſe ſeems worthy of obſervation, that the plants raiſed by the gardiners by grafting or inarching the andrachne upon the common arbutus, which is the method chiefly uſed in propagating this elegant ſhrub, differ conſiderably from the plants raiſed from ſeed, particularly in this, that the young branches, and the footſtalks of the leaves, are very hairy, and the leaves themſelves are all without exception deeply ſerrated like the arbutus. Dr. Ruſſell alſo informs me, that the outer bark of the old ſtem and branches abroad, are for ſome months of the year of as beautiful a crimſon, as the young ſhoots are here deſcribed to be, and doubts not but it will be ſo in this country, as the ſhrub grows older *.

* It may not be improper to mention, that the flower ſpike above deſcribed, with the glandular prominences, which were the rudiments of future flowers, made their appearance ſoon after Midſummer 1765: they advanced very ſlowly during the remains of ſummer; ſtood the winter under a ſlight cover, and made no great progreſs, till within a month of their flowering.

That plant, which produced theſe flowers, was one of ſeveral, which J. Gordon, of Mile-end, was fortunate enough to raiſe from ſeed, ſent by Dr. Ruſſell from Aleppo in 1754; and that this ſhould be the only plant which has hitherto produced flowers, is probably owing principally to its having been divers times tranſplanted.

J. F.

XIV. *Hiſtory*

Received December 20, 1766.

XV. *History of a Fœtus born with a very imperfect Brain; to which is subjoined a Supplement of the Essay on the Use of Ganglions, published in Philos. Trans. for* 1764: *By* James Johnston, *M. D.*

Read March 5, 1767.

IN october 27, 1765, a monstrous birth was brought me by a midwife of this place. It was a female child come to its full time, in which the whole scull excepting its basis was wanting: this was covered with something which had the appearance of red flesh. I found it to consist of different membranes; and in a small depression, in a back part of the basis of the skull, lay the brain, such as it was, not exceeding the size of the kernel of a filberd nut, flaccid and membranous. I could not have positively pronounced it brain, had I not traced its continuation into spinal marrow, down the channel of the vertebræ. The eyes were perfect and sound. The optic nerve of one eye I examined, though not large enough, yet in thickness was almost equal to one third of the spinal marrow, which was too small likewise.

Upon opening the breast and abdomen, all the organs contained therein seemed in structure perfect, properly situated and full grown. The heart in particular was plump and strong. This infant had not breathed,

breathed; its lungs, which were perfect, funk in water: yet the mother and midwife felt it active and ftrong juft before delivery.

This child had tongue, noftrils, eyes, and ears, and every other part, excepting the brain, perfect and plump, as in the healthieft infants come to their full time.

Many births fimilar to this, in moft circumftances, are recorded in the Tranfactions of the Royal Society, N°. 99. 226. 228. 242.

1. Such of them as were born alive, died foon after birth, though lively and ftrong in the womb, and perfect in all parts, the brain and fcull excepted.

2. In that of which an account is given by Dr. Prefton (Philof. Tranf. N°. 226.), the celebrated anatomift Monf. du Verney traced the eighth and ninth pairs, the medulla fpinalis, and the intercoftals. The child was well proportioned, the cranium, brain, and cerebellum were wanting; in lieu thereof, remained only a fubftance, like congealed blood, covered with a membrane.

3. In a cafe related, and largely commented upon, by the celebrated Wepfer *, which differs in many refpects from other children faid to be without brains; the child was well proportioned, its head of the ufual fize, but its brain had degenerated into veficles, or hydatides, each of which had its blood veffel (might one from thence infer the natural ftate of the cortical fubftance of the brain to be cellular?) and the optic and auditory nerves took their rife from three portions of medullary fubftance lying upon the fphænoid bone near the fella equina.

* Manget Biblioth. Auct. Vol. II. p. 339.

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4. These singular existences afford useful inferences, and shew that the irritability of the heart, is capable of being sustained, by very low degrees of the nervous power, while that irritability is kept up by the fostering heat of the mother. This feeble life is soon extinguished, when the influences of the mother's warmth and circulation cease (N°. 1). Such infants die as soon as born, or soon after.

5. Such examples more consequentially than experiments demonstrate that the spinal marrow is the principal origin of the intercostal nerves (N°. 2.); and better than ligatures illustrate their vast importance. for,

6. From the plump state of the body, and vigorous appearance of the heart, it is evident the circulation, and the developement of the several organs, had been carried on properly in the fœtus; and that the irritability of the heart derived a sufficiency of nervous influence from the intercostal nerves, and its ganglions, and these again from the spinal marrow, for growth, and that state of existence.

In the essay which was published in the Philos. Trans. for 1764, I endeavoured to prove, that as ganglions are seated constantly on the intercostal nerves, and on others sent to muscles whose motions are involuntary, and are very rarely seen on nerves sent to voluntary muscles, and not at all on the sensory nerves; it seems that, by means of ganglions, the motions of the heart and intestines and uvea are rendered uniformly involuntary. I was then, and am still sensible that various strong objections may be made to this doctrine, in common with every other system whatever; but especially every system which pretends

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to explain any thing relating to ſo obſcure a part of the animal œconomy, as the nerves, and their faculties. But as this doctrine, weighed againſt what has been ſaid againſt it, ſeems to me to have a conſiderable preponderance of evidence and probability in its favor, I ſhall now ſtate the ſtrongeſt objections which have been oppoſed to it, and endeavour to anſwer them.

1. The chief objection which has been made to this doctrine ariſes from obſerving, that the ſpinal nerves, have each one *ganglion:* and that one or two have been obſerved *ſometimes* upon the ſubdiviſions of the fifth pair of nerves.

With reſpect to the firſt, it is in the higheſt degree probable that the *ganglion* obſerved upon each of the ſpinal nerves reſpects ſolely the intercoſtals (ſee Winſlow's deſcription of that nerve, Exp. Anat. p. 462.) and is there ſeated to ſet apart for the uſes of the great ſympathetic nerves the *ſurculi*, which are from each of theſe ganglions detached to that great pair of nerves. 1. Becauſe the ſpinal nerves have no other *ganglion* in any part of their courſe. 2. When nervous twigs are ſent off from the ſpinal nerves, to join others beſides the *intercoſtals*, they have no *ganglions*; as the *nervi acceſſorii* ſent from the upper ſpinal nerves to join the eighth pair: and the firſt of the ſpinal nerves, and the laſt of the *nervi ſacri* are repreſented by Vieuſſens * as being without *ganglions*, and ſending no contribution to the intercoſtals.

Were the *ganglions* only little knots indiſcriminately ſuitable for nerves, which carry the commands of

* Neurograph.

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the will, and thoſe which do not, one might expect them equally frequent on the nerves of the limbs, as at the riſe and on the courſe of the *intercoſtals*, where they are numerous, large and conſtant: on the nerves ſent to the *ſenſory organs*, and on the *phrenetic* as well as the *lenticular ganglion* from which the Iris is provided with nerves. In a word the ſolicitude, ſo to expreſs myſelf, and the conſtancy with which all parts whoſe motions are involuntary are provided with nerves furniſhed and beſet with *ganglions*; and the great ſcarcity and rarity of them on nerves detached to muſcles ſubject to our volitions, and the total want of them on the ſenſory nerves, ſufficiently beſpeaks their general diſtinction and uſe, notwithſtanding a few ſeeming exceptions. I ſay ſeeming, becauſe ſome that are alledged as exceptions are not permanently and conſtantly found. To aſcertain the uſe and importance of any part of animal ſtructure, we ought to be certain that it is conſtantly found in that ſituation; if, on the contrary, it be only accidentally ſeen, and not perpetually, we can neither aſſign any important uſe to it, or draw any important doctrine from it, and have great reaſon to ſuſpect it to be rather ſome morbid phænomenon than otherwiſe. Now this is the caſe with reſpect to the *ganglia* deſcribed by Mekelius as ſeated on twigs of the ſecond and third branches of the fifth pair of nerves; betwixt which and other *ganglions* Baron Haller makes a very eſſential diſtinction, particularly the *ganglion ophthalmicum*, which he ſays is conſtant and perpetual, whereas theſe beforemention-
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ed are not ſo, for he * mentions his having examined bodies in which they were wanting.

But, ſuppoſing the utmoſt in favour of theſe *ganglia* of the fifth pair: the nervous twigs on which they have been obſerved are chiefly diſtributed to the ſalivary and mucous glands, about the tongue, jaw, palate, throat, and noſtrils, and therefore may be ſuppoſed to have ſome uſe in glandular ſecretion; for we ſee the glandular parts in the *abdomen* are ſupplied by the *intercoſtals* as well as the muſcular fibres of the heart and inteſtines.

2. It has likewiſe been objected that the *intercoſtals* ſend ſome branches to parts under the controul of the will as the *pharynx* and *diaphragm*; as well as to the heart and inteſtines, not ſubject to that controul.

It is well known that the *pharynx* has its moſt conſiderable ſupply of nerves, from the eighth pair: and the *diaphragm* is rendered paralytic by tying or cutting the *phrenic* nerves diſtributed to it, which ſhews that its motions have very little if any dependance on the minute filaments, which it receives from the *intercoſtals*. The motions however of both theſe parts are properly ſpeaking of the mixed kind, ſometimes being *voluntary*, at other times *involuntary*: thus the *diaphragm* moves when we are aſleep, as well as when we wake, and continues for ſome time, even during a profound apoplectic fit: and though we can raiſe the *pharynx* by an effort of the will, yet in the action of deglutition its motions are chiefly *involuntary* from the *ſtimulus* of the food, paſſing down the gullet, as has been ſhown in the moſt ingenious

* Haller, El. Phyſ. T. IV.

work of a great and worthy man lately deceaſed, an *eſſay on the vital and involuntary motions of animals*, by Dr. Whytt *.

And it deſerves to be remarked, that parts, whoſe motions are of this mixed kind, will be found to have generally a double diſtribution of nerves, namely ſuch as *are without ganglions*, to ſubject them to the *will*; and ſuch as *have ganglions* occaſionally to ſupport thoſe motions of the ſame parts which go on without the will; but, theſe ſupplies from the *intercoſtals* being very minute, their action is generally called forth and aſſiſted by ſome degree of uneaſy ſenſation or ſtimulus.

If theſe anatomical objections have not force enough to overthrow our doctrine, the following of a phyſiological nature, it is preſumed, will not be more formidable.

3. It has been objected, that if the *ganglia* intercept the communication between the *ſenſorium commune* and thoſe parts whoſe nerves are derived from them, they ought not only to intercept the commands of the will, and render the motion of theſe parts not voluntary, but they ought alſo to prevent the impreſſions made on the nerves of theſe parts from being conveyed to *the ſenſorium commune*, *i. e.* theſe parts ought to be inſenſible. The contrary of which is true; for example, the inteſtines, whoſe nerves come from *ganglia*, are among the moſt ſenſible parts of the body. And if the uneaſy ſenſation in the lungs, in aſthmatic caſes, was not conveyed to the *ſenſorium commune*, how could the will redouble the action of the *diaphragm* and the *intercoſtal* muſcles?

* P.

To this I anſwer, that the interruption of the parallel direction of the nervous filaments, which probably takes place in *ganglions*, may intercept the efforts of the will, and alſo render the ſenſations of parts wholly ſupplied with nerves from *ganglions*, more indeterminate and confuſed than in other parts; which in fact is the kind of ſenſation proper to theſe parts, yet without rendering ſuch parts totally inſenſible, which is well illuſtrated by morbid caſes. Paralytic diſeaſes ſhew that the nerves may be ſo affected as to become incapable of conveying the commands of the will, and yet remain ſufficiently capable of re-conveying ſenſible perceptions. In the palſies which are moſt frequent, the parts rendered immoveable by the diſeaſe have as quick a feeling as thoſe that remain moveable by the will, and, what by the way deſerves attention, are often moved involuntarily, eſpecially upon the application of any painful *ſtimulus*: and it is obſervable that the paralytic limbs, which are not to be moved by our volitions, are often called into action, when the paralytic perſon is ſuddenly thrown into ſome vehement paſſion: juſt as we obſerve the ſame cauſe to produce extraordinary commotions in the heart and inteſtines, &c. notwithſtanding the will, coolly exerted, has no power over theſe parts.

Various obſervations ſhow that the feelings of parts whoſe nerves come from *ganglia* are by no means acute, but blunt and confuſed. We have it on the authority of the great Harvey, confirmed by the experiments of Baron Haller, that the heart, though highly irritable, is yet when touched hardly ſenſible of it. Dr. Haller aſſerts that the lungs, liver, ſpleen

and kidneys, all ſupplied with nerves from the *inter-coſtals*, have been cut in pieces without the animal's ſeeming to feel pain. And what is leſs liable to exception, operations and diſeaſes in the kidneys, and ulcers in the lungs, ſhew their feelings not to be exquiſite *.

The ſtomach, which has a very large portion of the eight pair of nerves beſtowed upon it, by ligatures of this nerve, loſes its ſenſibility and contractive power ſo perfectly, that the food neither paſſes down the œſophagus, nor is concocted in the ſtomach, but, by ſpontaneous corruption there, puts on the appearance of the fæces themſelves in the great inteſtines †. This proves what was aſſerted concerning the eighth pair, as being a ſenſory nerve; and 'tis in conſequence of the ſenſibility which the ſtomach derives by means of this nerve, as well as its own ſtructure, that the ſtomach becomes the principal ſeat of hunger: "And (to uſe the words of Dr. Whytt ‡), as it is "affected with a more diſagreeable ſenſation, when "we have wanted food for any conſiderable time, than "the guts, ſo likewiſe it is more ſenſible of an agree-"able feeling from grateful food, and in theſe reſpects "it may be ſaid to be more ſenſible than the inte-"ſtines." Baron Haller has obſerved that parts which have nerves from *ganglions* are not ſo diſtinctly painfull as others, "ut anima non adeo accurate locum "dolentem diſtinguat, ſed obiter utcunque, et cum "aliqua latitudine §." And this confuſed indeter-

* Haller. Irritab. et Whytt. Path. Eſſays.
† See Vieuſſens, Bruni, Morgagni, Haller.
‡ Path. Eſſ. p. 155.
§ Elem. Phyſ. T. IV. p. 407.

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minate ſenſation is the ſenſation proper to the inteſtines, though in many inſtances they are the ſeat of exquiſite pain; yet, in conſequence of the concourſe and commixture of the nervous filaments in *ganglia*, any painful diſeaſe ſeated in the inteſtines, or in others of the viſcera contained in the *abdomen*, is leſs determinable to its particular ſeat, or rather is more apt to affect the parts contained in the *abdomen*, not primarily affected, than diſeaſes of a painful nature, which are ſeated in the ſtomach itſelf, or other parts whoſe nerves are unſupplied with *ganglions*. And this leads to a natural ſolution of the cauſe of that ſympathy, that communion of ſenſation, or imputation of ſenſation, which ſo frequently takes place, in the diſeaſes of the contained parts of the abdomen, from which ſome writers (Linn.*) have very concluſively argued for the neceſſity of ſuch a communication of the nervous filaments in *ganglia* as we contend for, from the beſt anatomical authority, and which appears to have ſuch important uſes in the animal œconomy, and to be the occaſion of that ſympathy or confuſion of ſenſation among the *abdominal viſcera* in particular †.

* In Haller. El. Phyſ. T. IV. p.

† The ſolution of the problems concerning the ſympathetick affections, or conſent of parts, has employed the hands and pens of many ingenious writers; and if all the queſtions relating to it were diſcuſſed, volumes might be filled, and the ſubject neither exhauſted nor underſtood. The ingenious Dr. Whytt has with great acuteneſs ſhewn that ſympathy in general is only to be accounted for from a ſentient principle, ſeated in the *ſenſorium commune*, where all nerves begin, and communicate; his objections to particular ſympathies ariſing from a connexion of nerves in *ganglions* ſeem inconcluſive; for he remarks that ſuch a communication as is ſuppoſed in *ganglia* to occaſion ſympathy,

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4. It is objected alſo that every voluntary muſcle in the body becomes involuntary when it is ſtrongly ſtimulated, for example the *acceleratores urinæ* are quite voluntary in their action of expelling the urine; but act involuntarily in expelling the *ſemen*.

When we conſider the ſtate of the ſoul and body under any great commotion of mind, we find the uſual operations of the mind itſelf are not only interrupted, but thoſe parts of the body too which the will cannot controul are now agitated by the ſtorm; for every one has experienced that the heart and *viſcera* in general are vehemently affected by ſtrong paſſions. The mind is in its turn re-acted upon, by very ſtrong bodily ſenſations; it being well known that muſcular parts, which are ordinarily ſubject to our volitions, ceaſe to be ſo if any part is ſtimulated by exquiſitely pleaſing, or exceſſively painful ſenſations; under ſuch a *ſtimulus*, they are neceſſarily contracted or convulſed. But it is not therefore to be concluded that the gentle *ſtimulus* of the blood on the ſurface of the heart, and of the air, food, and inteſtinal juices on the inteſtines, of which the mind has no conſcious perception at all, much leſs a diſagreeable one, can lay it under any ſimilar neceſſity, as ſome have argued; and therefore the independence

would cauſe a confuſion in our ſenſations as well as in the motions of our muſcles.—with reſpect to ſenſation I have remarked that confuſion or indeterminate ſenſation, is that, which is proper to parts whoſe nerves ariſe from *ganglions*—and that the muſcular motions of theſe parts are by means of *ganglions* not regulated by the will, but ſubſiſt by the application of an irritating cauſe: and, inſtead of proving that ſympathy in the *abdominal viſcera* does not ariſe from *ganglions*, they prove that it does. See Whytt on diſeaſes of the nerves.

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of theſe motions on our minds, cannot be explained from this analogy, which does not ſubſiſt in fact. This objection therefore can have no force againſt our doctrine, however it may recoil upon another; as all voluntary muſcles whatever may be excited to contractions by irritations exceſſively painful or pleaſing, the contraction in ſuch caſes being involuntary, neceſſary, and uncontroulable. But the *ſtimuli* that affect the heart and other parts, whoſe motions are naturally involuntary, are not of this claſs and ſtrength, and indeed are ſo little perceived by the mind, as to lay it under no ſuch neceſſity, as that *ſtimulus* which renders the action of the *acceleratores urinæ* involuntary in expelling the *ſemen.*

5. Laſtly it has been objected, that, though the motions of the *uvea* are involuntary from light affecting the eye; they are truly voluntary when it contracts in order to the diſtinct viſion of an object placed near the eye, whoſe minute parts we want to obſerve accurately.

It ſeems on the contrary certain, that the contractions of the *uvea*, in order to diſtinct viſion, equally ariſe from different impreſſions of light on the *retina*, and are equally involuntary, and ſolely dependant upon the impreſſions of different degrees of light in all caſes: the diſtinction indeed is chiefly verbal; as it is granted, that the motions of the *uvea* are involuntary from light affecting the eye, that is, different degrees of light ſtriking upon the *retina* neceſſarily occaſion more or leſs of contraction in the pupil. But as viſion, conſidered as diſtinct or indiſtinct, is occaſioned by various impreſſions of light upon the *retina*, the contraction of the pupil neceſſarily fol-

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lows, according to the degree of that impression: in the case of distant objects, the faint impressions of light on the *retina* make the pupil contract little, and it remains wide; for dilatation is the natural state of the pupil. In observing very near objects, the light is *cæteris paribus* stronger, and stimulates the *retina*, and contracts the pupil more.

In a word, the contractions of the *uvea* arise from the sensations of the *retina* involuntarily and uniformly, according to an invariable law and connexion; otherwise why does the pupil constantly become immoveable, when by a *gutta serena* the *retina* becomes insensible? Let any one observe the motions of the pupil, by the help of a mirror, they will always find it impracticable to subject them to the will. Indeed it is clear from experiments, as well as diseases, that the *iris*, like all other parts provided with nerves from *ganglia*, has but a dull degree of feeling, and is moved entirely independent of the will *. "What persuades me, says M. de Haller, "that the *iris* is much less sensible than the *retina*, "is, that if, after having pierced the *cornea*, you irritate or cut the *iris*, it is not therefore contracted, "whereas the least increase of light makes it contract; which evidently proves that this contraction "does not depend upon the proper sensibility of the "*iris*, but on that of the *retina*. The *gutta serena* "serves to prove the same thing, the *iris* being no "ways changed in that disease, any further than it "is deprived of motion, from the sensation of the "*retina*, being destroyed by a palsy of the optic "nerve." Essay on Irritability, p. 31.

* See Whytt, Ess. on the Invol. motions. Le Cat, on the Senses. Haller, Elem. Phys. &c.

To

To conclude, the *ganglia*, refpecting their ftructure, may juftly be confidered as little brains, or germs of thofe nerves detached from them, confifting, according to Winflow, of a mixture of cortical and nervous medullary fubftance, nourifhed with feveral fmall blood veffels *, in which various nervous filaments are collected, and in them lofe their parallel rectilineal direction, according to Baron Haller †, who likewife obferves that *ganglia* fend off more and larger nerves then came to them ‡; fo that a new nervous organization, analogous to the brain, probably takes place in them.

Refpecting their ufes, *ganglions* are the origins of the nerves, fent to organs, moved involuntarily, and probably the caufe, or check, which hinders our volitions from extending to them. As different fources of nervous power, *ganglions* are analogous to the brain in their office, though they derive their nervous filaments (to be new arranged in them), and confequently their power, ultimately from it.

In a word, *ganglions* appear to limit the arbitrary power of the foul in the animal œconomy.

They put it out of our power, by a fingle volition, to ftop the motions of our heart, and in one capricious inftant irrevocably to end our lives: and however in the dark we may be, what fubordinate agents are fubftituted, fo uniformly to guide and direct, independent of us, our vital and involuntary motions; we muft at leaft clearly difcern, in the contrivance, the goodnefs, boundlefs, and unerring wifdom, no lefs than the power, of our adorable Creator! "ad "impellendum fatis, ad docendum parum."

* Winflow, Traité de la Tefte, 629. † Halleri Elem. Phyf. T. iv. p. 203. ‡ Haller, ibid.

Received January 14, 1767.

XVI. *Cogitata de* COMETIS. *Communicated by* Benjamin Franklin, LL.D. F. R. S.

Illuſtriſſimæ SOCIETATI REGIÆ,

Ob ſummum in ſe collatum honorem,

Cooptationem ſcil. in eorum Sodalitium,

Hanc qualemcunque Diſſertationem,

Ea qua par eſt obſervantia,

D. D. C. Q.

Johannes Winthrop,

Apud Cantab. Nov. Ang. Math. & Phil. Prof. Hollisianus,

7° Maii 1766.

L E M M A.

Read March 19, 1767.

CUM ex illuſtriſſimi Newtoni inventis conſtet, " gravitatem in " univerſa corpora fieri, eamque proportionalem eſſe " quantitati materiæ in ſingulis, et reciproce propor- " tionalem quadrato diſtantiæ inter corporum centra," exinde ſequitur, quòd inter bina quævis ſyſtematis mundani corpora exiſtere poteſt *limes* attractionis, in

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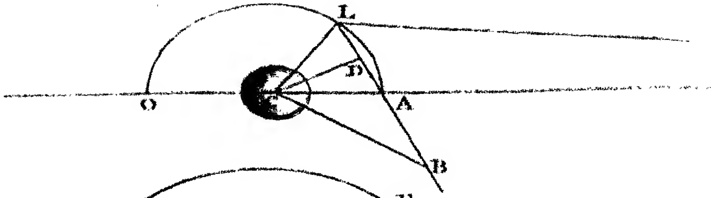
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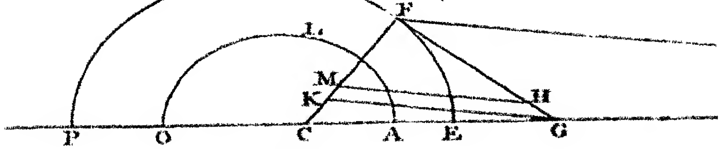
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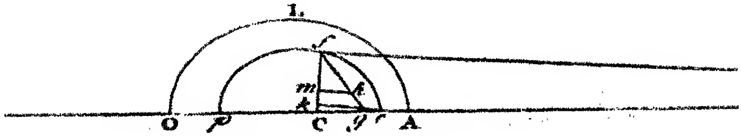
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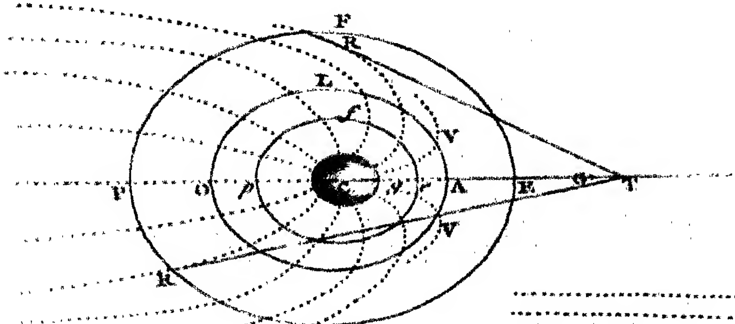
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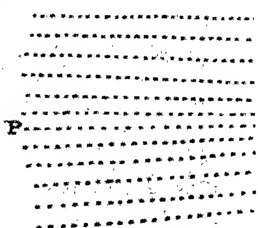
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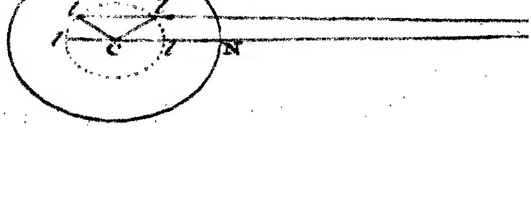
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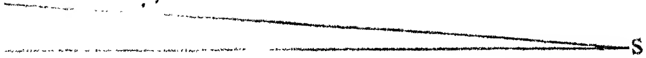
Fig. 1.



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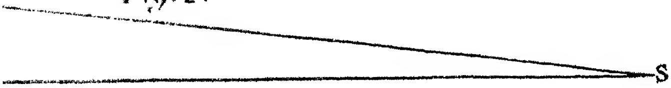


Fig. 3.

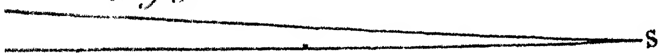


Fig. 4.

Fig. 5.

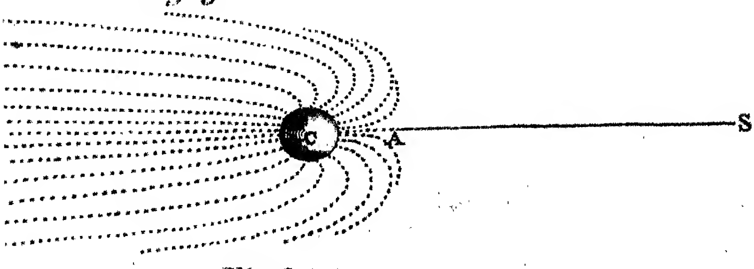


Fig. 6.

quo utique ſitum corpuſculum æqualibus viribus utrumque verſus urgebitur.

Invenire hunc limitem inter ſolem et cometas, et quædam inde pendentia, eſt ſcopus problematum ſequentium.

PROB. I.

Datis, materiæ quantitatibus in duobus corporibus, et diſtantiâ inter eorum centra; invenire limitem attractionis.

TAB. VII.

In Fig. 1. ſint S et C centra corporum, quorum majus, S; et quantitates materiæ in ipſis vocentur *s* et *c* reſpectivè. Secetur recta SC, ultra minus corpus C in infinitum producta, in A et O, ita ut ſint SA ad AC, et SO ad OC, in ſubduplicata ratione ipſius *s* ad *c*; ſuperque diametro OA deſcribatur ſemicirculus OLA: Et limes attractionis erit ſphærica ſuperficies circumactu ſemicirculi OLA circum axem OA genita.

Nam ex iis quæ præmiſſa ſunt in lemmate patet, puncta A et O eſſe in limite. Et, ſi a puncto quovis L in ſemicirculo OLA ducantur rectæ LS, LC ad centra corporum S et C, erit, ex natura circuli, SL ad LC ut SA ad AC, et ut SO ad OC; unde punctum L eſt in limite. Pari ratione, omnia puncta in ſemicirculo OLA, adeoque in ſphærica ſuperficie circumactu iſtius ſemicirculi genita, ſunt in limite quæſito. Limes igitur attractionis eſt hæc ſuperficies ſphærica, corpori minori C eccentrica. Q. E. I.

SCHOL.

SCHOL. I.

Intra hanc ſuperficiem, quam voco *limitantem*, corporis minoris vis plus pollet: Extra, majoris.

Corol. I. Sphæræ limitantis diameter AO, ejuſque ſegmenta AC, CO, ſunt ut diſtantia inter corporum centra.

Corol. 2. Dato quovis puncto in ſuperficie limitante, ut et corporum diſtantiâ, datur tota ſuperficies.

Corol. 3. In hac ſuperficie, gravitatio dirigitur ad punctum A, tanquam ad centrum. Ob æqualitatem virium, quibus corpuſculum in L trahitur verſus corpora S et C, directio vis ex ipſis compoſitæ bifariam ſecat angulum SLC; ideoque tranſit per punctum A; per iii. 3. elem.

Corol. 4. Et ducto perpendiculo CB ad CL, occurrente ipſi LA (productæ, ſi opus ſit) in B; vis ipſa compoſita erit reciproce ut rectangulum CLB. Nam CD demiſſo perpendiculo ſuper LA, vis ſimplex verſus C erit ad vim compoſitam verſus A, ut CL ad 2LD, id eſt, ut BL ad 2CL. Unde, cum vis ſimplex ſit ut $\frac{1}{CLq}$, vis compoſita erit ut $\frac{2CL}{CLq+LB}$, ſive ut $\frac{1}{CL \times LB}$.

SCHOL. II.

Si duo corpora fuerint æqualia, limes attractionis eſſet planum infinitum, diſtantiam corporum bifariam et ad rectos angulos ſecans. In hoc caſu, CA æquaretur ipſi AS, et punctum O abiret in infinitum.

SCHOL.

S C H O L. III.

Positis, distantiâ $SC=d$; semidiametro corporis majoris $S=b$, et minoris $C=k$: si ea fuerit corporum distantia, ut sit $d:k::\sqrt{s}+\sqrt{c}:\sqrt{c}$, punctum A continget superficiem corporis C. Idem eveniet puncto O, si, imminutâ paululum distantiâ, sit $d:k::\sqrt{s}-\sqrt{c}:\sqrt{c}$. Sin distantia d adhuc minor fuerit, Problema evadet impossibile.

P R O B. II.

Iisdem positis, invenire locum in quo vires corporum sint ad invicem in ratione data.

Sit ratio data b ad c, in qua oportet esse vim corporis majoris ad vim minoris. Secetur producta SC (Fig. 2.) in E et P, ita ut sint SE ad EC, et SP ad PC in subduplicata ratione ipsius s ad b; et locus quæsitus erit superficies sphæræ PFE, diametro PE descriptæ. Q. E. I.

Demonstratur ut Prob. I.

Corol. 1. Si secetur CS in G, ita ut sit CG ad GS, ut $\frac{b \times CE}{c}$ ad ES, punctum G erit centrum ad quod dirigetur composita gravitatio in superficie PFE. Jungantur FS, FG, FC; et agatur recta GK ipsi SF parallela. Cum ratio CG ad GS, sive CK ad KF, componatur ex rationibus CK ad KG, (id est, CF ad FS, sive CE ad ES) et KG ad KF; et, per constructionem, ratio CG ad GS componitur ex rationibus CE ad ES, et b ad c; consequens est, quod

KG

KG eſt ad KF ut *b* ad *c*; id eſt, ut vis corporis S agens ſecundum rectam ipſi KG parallelam, ad vim corporis C agentem ſecundum rectam FK.

COROL. 2. Et, ſi in diagonali FG ſumatur FH = FC, et agatur HM ipſi SF parallela, vis compoſita in puncto F erit reciproce ut rectangulum CFM.

Demonſtratur ut Corol. 4. Prob. I.

Eadem intelligenda ſunt de ſuperficie interiori *pfe*, et punctis *g*, *k*, *h*, *m*; in Fig. 3.

COROL. 3. Ubi *b* minor eſt quam *c*, centrum *g* verſatur intra ſuperficiem *pfe*, ut in Fig. 3. Ubi major, centrum G verſatur extra ſuperficiem PFE; eoque longius diſtabit a corpore C, cæteris manentibus, quo major fuerit ratio data.

Cæterùm (ut id obiter moneam), vires conjunctæ gravitatis non in diverſis ejuſdem ſuperficiei partibus tantùm, ſed et in diverſis ſuperficiebus, ſunt inter ſe in ratione ſupradicta. V. gr. Gravitas in puncto F eſt ad gravitatem in puncto *f*, ut rectangulum *Cfm* ad rectangulum CFM; in Figg. 2. et 3.

S C H O L.

Si ratio data eadem ſit ac *s* ad *c*, ſphærica ſuperficies PFE in planam mutabitur; haud ſecus ac in Schol. 2. Prob. I. puncto P in infinitum abeunte. Si ratio fuerit major, punctum P cadet in contrariam partem centri S; et ſuperficies iterum erit ſphærica, at corpori majori eccentrica; ejuſque diameter invenitur

nitur ut supra. Sin ratio data fuerit major quàm $\overline{b+d}^2 \times s$ ad b^2c; vel minor quam k^2s ad $\overline{k+d}^2 \times c$; Problema erit impossibile.

PROB. III.

Corpusculorum, conjunctis corporum S et C viribus attractorum, motus generatim describere.

Si corpora S et C medio fluido circumdentur, in quo mergantur corpuscula specifice leviora aut graviora quàm istud medium, corpuscula illa perinde ascendent vel descendent, per utriusque corporis attractionem, ac si ad corpus unicum traherentur; ideoque movebuntur vel in rectis lineis vel curvis, prout eorum motus directi sint vel obliqui, respectu centri compositæ gravitationis. Nam centrum hoc idem valet * ac corporis unici centrum in eodem puncto locatum.

Cas. I. Corpuscula inter corpora C et S in recta CS sita, quæ specifice leviora sunt medio ambiente, tendunt ad punctum A, Fig. 1. Nam quæ inter corpus C et puuctum A sita sunt, ascendunt a corpore C; et quæ inter corpus S et idem punctum, a corpore S ascendunt, (per Schol. I. Prob. I.) Corpusculum autem in ipsissimo puncto A situm, in æquilibrio detentum, requiescit. Quæ in recta CO sita sunt, ex altera parte corporis C, ascendunt ultra limitem O ad altitudinem indefinitam. Hæc enim, in toto itinere, quantumvis longo, ascendunt simul ab utroque corpore C et S. Contra fieret, in corpusculis specifice gravioribus: quod et de casu sequente dicendum.

* Hæc mathematice dicta sunt, non physice. Nam centra minime trahunt.

Cas. II. Omnia corpuſcula leviora, e corpore C oriunda, iis quæ in recta ſyzygiarum PS ſita ſunt exceptis, aſcendunt in curvilineis ſemitis, non multum diſſimilibus, quantum auguror, eis quæ punctis ſignantur, in Fig. 4. quarum convexitas obvertitur corpori majori S, et quæ magis magiſque tendunt verſus plagam ei oppoſitam. Leviora enim, quæ in ſuperficie limitante OLA ſita ſunt, aſcendunt a puncto A; et quæ in ſuperficie PFE, vel *pfe*, (Figg. 2. et 3.) a puncto G, vel *g*; quoniam hæc puncta ſunt centra gravitationis compoſitæ, ad corpora C et S; per Corol. 3. Prob. I. et Corol. 1. Prob. II. Ejuſmodi corpuſcula, cum primum expedita ſunt a corpore C, aſcendunt quaquaverſum ab ipſius centro, ſaltem quam proxime; peragrando autem ſuperficies *pfe*, PFE, &c. aſcendunt quaſi depulſa a centro *g*, vel G, &c. quod ſemper jacet inter puncta C et S; et, dum augetur ratio *b* ad *c*, manente diſtantia CS, perpetuo recedit a corpore C; per Corol. 3. Prob. II. et citius, auctâ quoque diſtantiâ CS. Quamdiu intra ſphæram limitantem comprehenduntur, aſcendunt fere a corpore C. In tranſitu enim per ſuperficiem interiorem *pfe*, fugantur a centro *g*, quod inter C et *e* locatur; at, ob compoſitionem motûs antea acquiſiti cum niſu aſcensûs a centro *g*, directiones in quibus aſſurgunt, ſeu tangentes ſemitæ ſuæ, ſecant rectam C*g* in punctis quæ adhuc propiora ſunt corpori C quàm eſt punctum *g*. Egreſſis extra ſphæram limitantem, et ſuperficies exteriores PFE permeantibus, aſcenſus eorum magis magiſque fit a corpore S. Fugantur nunc a centro G, quod locatur inter E et S; eoque magis appropinquat, cæteris paribus, corpori S, quo altius aſcenderunt corpuſcula a corpore C.

Corol. 1. Corpuſcula, quæ a corpore C prope rectam ſyzygiarum CS aſſurgunt, ubi regiones ipſi A vicinas attigerunt, ſeſe ad latera diffundent, curſumque in partes contrarias flectent; velut aqua fontis arte fabricati, ſimul ac ſummam conſecuta eſt altitudinem, quaquaverſum diffluit, retrorſus jamjam itura. Et omnium quidem ſemitis corpuſculorum ex toto corporis C hemiſphærio ipſi S proximo ſuſcitatorum competit vertex V, Fig. 4. ſeu punctum ex quo curva in contrarium producitur. Cis hunc verticem, corpuſcula ad corpus S accedunt; trans, ab ipſo recedunt. Quæ ab hemiſphærio oppoſito ortum ducunt, ab ipſo S nunquam non recedunt.

Corol. 2. Recedentibus corpuſculis a corpore S, id eſt, trans verticem V, angulus RTC, (Fig. 4.) ſub ſemitæ tangente RT et ſyzygiarum recta CS contentus perpetim minuitur; ad modum parabolæ.

Corol. 3. Vis, quâ corpuſcula leviora in his ſuperficiebus ſphæricis ſita aſcendunt, eo major eſt quo propiora ſunt illa plagæ oppoſitionis, pOP. Manentibus enim medii ambientis et corporis immerſi denſitatibus, ſi augeatur vis acceleratrix gravitatis in quacunque ratione, augebitur in eadem ratione differentia gravitatum ſpecificarum, id eſt, vis qua corpus immerſum ſurſum vel deorſum fertur in iſto medio. Augetur autem vis acceleratrix in unaquaque harum ſuperficierum, pergendo a conjunctione CE per F ad oppoſitionem CP; † per Corol. 4. Prob. I. et Corol. 2. Prob. II.

† Vis acceleratrix augetur in ſuperficie quacunque EFP, ab E ad F et P, quamdiu ratio data in Prob. II. minor fuerit quam $\sqrt[3]{\frac{s}{\pi}}$ ad 1, poſita $c = 1$. Si verò hæ rationes æquentur, vires in E et P erunt, non accurate quidem ſed quàm proxime, æquales. Id quod ex Corol. 2. Prob. II. facile colligitur.

PROB. IV.

Ex ſupradictis, præcipua caudarum cometicarum phænomena derivare, in theoriâ Newtoni.

Secundùm hunc Philoſophum celeberrimum, caudæ cometicæ ad hunc modum formantur: "* Caudas a capitibus oriri et in regiones a ſole averſas aſcendere, confirmatur ex legibus quas obſervant. "† Suſpicor aſcenſum illum ex rarefactione materiæ caudarum oriri. Aſcendit fumus in camino impulſu aëris cui innatat. Aër ille per calorem rarefactus aſcendit, ob diminutam ſuam gravitatem ſpecificam, et fumum implicatum rapit ſecum. "Quidni cauda cometæ ad eundem modum aſcenderit a ſole? Nam radii ſolares non agitant media, quæ permeant, niſi in reflexione et refractione. "Particulæ reflectentes ea actione calefactæ calefacient auram ætheream cui implicantur. Illa calore ſibi communicato rarefiet, et ob diminutam ea raritate gravitatem ſuam ſpecificam, qua prius tendebat in ſolem, aſcendet et ſecum rapiet particulas reflectentes, ex quibus cauda componitur." Hæc eſt ſumma theoriæ Newtonianæ: Adjicit autem Auctor, "Ad aſcenſum vaporum conducit etiam, quod hi gyrantur circa ſolem et ea actione conantur a ſole recedere, at ſolis atmoſphæra et materia cœlorum vel plane quieſcit, vel motu ſolo quem a ſolis rotatione acceperit, tardius gyratur. Hæ ſunt cauſæ aſcenſus caudarum in vicinia ſolis, ubi orbes curviores ſunt, et cometæ intra denſiorem et ea ratione graviorem ſolis atmoſphæram conſiſtunt, et caudas quam longiſſimas mox emittunt."

* Newt. Princip. p. 511. Edit. tertiæ. † Id. p. 514.

Ex

Ex theoria jam expoſita, ſequentia corollaria levi negotio deducuntur.

Corol. 1. Caudæ cometicæ ad ſoli oppoſitum dirigi debent. Cùm ſol major eſt quàm cometa quivis, quæ in Problematibus ſuperioribus de conjunctis corporum inæqualium viribus earumque effectibus demonſtrata ſunt, hîc locum obtinent. Corpuſcula igitur, ex quavis parte capitis cometæ C excitata, eas, eaſque ſolum, attingere debent altitudines, ad quas a viribus ibi agentibus impelli poſſunt. Quæ verſus conjunctionem ſolis S excitata ſunt, attingere poſſunt ſphæram limitantem juxta A, non autem tranſgredi; per Caſ. I. Prob. III. Aliis ex partibus excitata, altius a capite aſcendere poſſunt, ſed eorum ſemitæ ſemper detorquentur verſus ſoli oppoſitum; per Caſ. 2. Prob. III. Et in ipſo oppoſito, rectà aſcendunt a capite ad altitudinem indefinitam ſupra O; per Caſ. 1. Prob. III. Præterea, corpuſcula ad maximas altitudines aſſurgent in ea plaga ubi vis ſurſum impellens eſt maxima. Hæc vis autem eſt maxima in plaga ſoli oppoſita; per Corol. 3. Prob. III. Proinde, minima caudarum altitudo (ſi ita loqui fas ſit) ſpectabit ad ſolem; et maxima, ad ſoli oppoſitum.

Corol. 2. Caudæ, ab extremitate inferiore ad ſuperiorem, dilatari debent. Nam directiones vaporum aſcendentium intra quamvis ſuperficiem, PFE, divergunt a punctis inter cometam, C, et centrum compoſitæ gravitationis, G, jacentibus; quæ puncta eo propius accedunt ad ſolem, S, quo altius aſcenderunt vapores a capite cometæ; ut in Caſ. 2. Prob. III. expoſitum eſt. Poſtquam igitur corpuſculum tranſivit ultra ſemitæ verticem, V, (Fig. 4.) dum a centro fugitivo, G, aſcendere conatur, directiones aſcenſûs ſu

ſemper vergent ad parallelifmum cum recta ſyzygiarum, SC, per Corol. 2. Prob. III. nunquam vero attingent.

Corol. 3. Et caudæ longiſſimæ eſſe debent in vicinia ſolis; idque ob ſequentes cauſas. 1. Ob majorem vaporum e cometa deinceps extractorum copiam, majori calori ſolis quodammodo proportionatam, ſive congruentem. Eſt autem hic calor reciproce in duplicata ratione diſtantiæ a ſole. 2. Ob majorem vaporum raritatem, ex eodem caloris gradu pendentem. 3. Ob majorem medii ambientis denſitatem. Nam materiam cœlorum, cujuſcunque demum ea ſit raritatis, denſiorem eſſe prope ſolem, ſeu commune centrum gravitatis ſyſtematis mundani (niſi quatenus rareſcat ingenti calore juxta ſolis ſuperſiciem) rationi conſentaneum videtur. Denſiores enim particulæ inferiora petunt loca; et inferiores ſuperioribus comprimuntur. Sed in qua ratione diſtantiæ a ſole denſitas illius medii varietur, id nondum eſt compertum. Ex hiſce duabus cauſis (2. ſcil. et 3.) junctim ſumptis, oritur major differentia gravitatum ſpecificarum, et inde major vis aſcensûs; quæ inſuper augetur. 4. Per auctam vim acceleratricem ad ſolem; ut in Corol. 3. Prob. III. notatum eſt. Hæc vis eſt reciproce in duplicata ratione diſtantiæ a ſole. 5. Ob diminutam ſphæram limitantem; quo pacto fit, ut complures vapores, qui in majoribus a ſole diſtantiis intra ampliorem ſphæram continebantur, et tunc aſcendebant fere a cometa, in minoribus extra contractam ſecluſi, aſcendant potiſſimum a ſole; per Caſ. 2. Prob. III. caudamque anguſtiorem efficiunt, at productiorem. Hæc autem diminutio fit fere * in tri-

* Non accurate; propter magnitudinem incluſi nuclei.

plicata

plicata ratione diminutæ distantiæ a sole; per Corol. 1. Prob. I.

Notandum nihilominus, non in ipso perihelio, sed paulo post, caudas fore longissimas. Nam, ob continuationem virium impressarum, effectus solent esse maximi, postquam eorum causæ aliquantum sunt diminutæ. Quemadmodum enim "*maxima altitudo "æstus marini non incidit in appulsus luminarium "ad meridianum, ubi vis eorum ad mare elevandum "maxima est, sed in secundam tertiamve horam po- "stea; pariterque æstas et hiems maxime vigent, "non in ipsis solstitiis, sed quasi triginta diebus "postea:" Sic caudæ cometicæ prolixiores esse debent, postquam cometæ perihelion sunt transgressi, et a sole recedere inceperunt.

Corol. 4. Ex iisdem rationibus concludi quoque potest, quòd cometæ caudatissimi erunt ii, cæteris paribus, qui proxime ad solem appropinquant.

Hæc omnia cum phænomenis congruere, notissimum est.

SCHOL. I.

Si lex vis centrifugæ, ex causis in Corol. 3. enumeratis (aliisque, si quæ sint) oriundæ innotesceret, daretur natura curvarum in Cas. 2. Prob. III. descriptarum, et inde figura caudæ cometicæ *a priori* (ut loquuntur) geometrice determinari posset. Impræsentiarum hoc tantum dicere ausim, quòd cauda induet figuram conoëidi cuidam (forsan parabolico) non dissimilem, cujus vertex solem respiciet, et axis protendetur in partes a sole aversas. Illius speciem, se-

* Newt. Princip. p. 424. & 466.

cundum

cundùm ſenſa mea, quodammodo adumbrare in Fig. 5. periclitatus fui *.

Hîc verò conſideravimus ſolum motum relativum, quo corpuſcula vaporis a capite quieſcente in altum aſſurgerent, et quo calefactæ partes auræ æthereæ, † quæ nullum alium habent motum, reverà aſſurgunt. Corpuſculis autem vaporis omnem retinentibus motum quem derivare poſſunt a capitis motu, ſive progreſſivo circa ſolem, ſive circulari circa axem proprium, motus iſte cum motu aſcensûs componitur; et ex illa compoſitione orientur incurvatio axis caudæ, et deviatio ab oppoſitione ſolis, quæ majores vel minores erunt pro velocitate et directione motûs ab hoc fonte derivati. Et hîc obiter animadvertendum occurrit, quòd cùm nulla alia cernitur unquam incurvatio aut deviatio, quàm quæ oriri poteſt a motu progreſſivo capitis circa ſolem, veriſimile eſt cometas circa axes non rotari.

SCHOL. II.

Caudæ a ſphæris planetaram abeuntis pars quædam ad planetas attrahi poteſt, modò ad aliquem eorum, intra debitam diſtantiam, accedat. Sed pars major, quantum ego quidem video, caput comitata in regiones a planetis aliiſque cometis quàm longiſſime diſſitas, refrigeſcet; et condenſata, in corpus unde originem traxit, per gravitatem ſuam paulatim recidet, et illud, tractu temporis, more atmoſphæræ undique circumfluet; ita ut, quum exacta periodo ad con-

* Similem cometæ cujuſdam obſervati imaginem ab Hookio depictam poſtea deprehendi.

† Vide Addend. I. p. 30.

ſpectum

ſpectum noſtrum redierit, inſtar coronæ caput æquabiliter cingentis videatur, in caudam denuo producenda. Si cometa quivis materiâ caudæ ſuæ ſpoliatus fuerit, nonne expectandum foret, ut cauda regenerata ſingulis revolutionibus contraheretur? Utrum verò res ita ſe habeat, id utique demonſtrandi nulla ſeſe hucusque obtulit occaſio.

PROB. V.

Invenire limitem attractionis inter ſolem et datum cometam, ex obſervatione.

Obſervetur latitudo capillitii, CA, in Fig. 5. a centro cometæ æſtimata, in ea parte quæ caudæ opponitur; et hinc, unà cum diſtantiâ cometæ tam a ſole quàm a terra, quæ dantur ex theoria gravitatis Newtoniana, habebitur punctum A, Figg. 1. 4. et 5. vel accurate, vel ſaltem quam proxime (præſertim ſi angulus ſub rectis a cometa ad ſolem et terram ductis contentus, ſit fere rectus). Inde verò, per Corol. 2. Prob. I. dabitur totus limes. Q. E. I.

Nam punctum A, ſive eam partem ſuperficiei capillitii quæ caudæ opponitur, eſſe in limite, patet ex iis quæ dicta ſunt in Corol. 1. Prob. IV.

SCHOL.

In hunc finem, neceſſe eſt cometam a ſole intra certos limites diſtare. In deſcenſu ab aphelio, tam prope ad ſolem accedere oportet, ut vaporis incaleſcentis columna aſſurgere incipiat, et atmoſphæræ forma rotunda mutari in oblongam. Hoc prius fieri nequit, quàm CA (Fig. 1.) minor ſit ſemidiametro

 atmoſ-

atmoſphæræ. In majoribus diſtantiis, cometa capillitio ornatur, caudâ verò deſtituitur; ut in Schol. 2. Prob. IV. Sin plus æquo appropinquet, coma a fronte capitis retro verſa et quaſi abraſa *, tota in caudam abibit. Sed hoc perpaucis cometis, iisque non niſi per breve tempus, evenire poteſt. In utroque caſu, nullus obſervationibus hîc poſtulatis relinquitur locus. Eadem dicenda ſunt de aſcenſu cometæ a perihelio. Limites autem ex hac parte axis tranſverſi orbitæ majori intervallo ab invicem diſtabant quàm ex altera. Ubi cometa prope ſolis oppoſitionem verſatur, hujuſmodi obſervationes peragi nequeunt.

Corol. 1. Latitudo capillitii, in ea parte quæ reſpicit ſolem, quamdiu cometa intra limites in Scholio præcedente memoratos verſatur, eſt ut diſtantia cometæ a ſole; per Corol. 1. Prob. I.

Et hoc cum phænomenis conſentire, auctorem habemus Newtonum, obſervationibus Hevelii fretum. "Atmoſphæræ cometarum, ait ille †, in deſcenſu "eorum in ſolem excurrendo in caudas, diminuun-"tur, et (ea certe in parte quæ ſolem reſpicit) an-"guſtiores redduntur; et viciſſim, in receſſu eorum a "ſole, ubi jam minus excurrunt in caudas, amplian-"tur: Si modo phænomena eorum Hevelius recte "notavit."

Corol. 2. Hinc innoteſcit quantitas materiæ in cometa. Nam cum dentur SC et CA, datur earum differentia SA; et inde datur ratio SA ad AC; ut et ratio hujus duplicata, quæ (per ſolutionem Prob. I.)

* Vide Schol. 3. Prob. I. et confer obſervata Hevelii de cometa anni 1665, die 20 Aprilis; infra citata in p. 148.

† Newt. Princip. p. 516.

eadem eſt ac ratio materiæ in ſole ad materiam in cometa.

Corol. 3. Hinc, et ex obſervata diametro, innoteſcit etiam denſitas cometæ. Nam denſitas ſphæræ eſt ut ejus materia directe, et cubus diametri inverſe.

Newtonus ex fœcundo gravitatis principio elicuit methodos determinandi denſitates ſolis, lunæ et planetarum ſatellitio ſtipatorum. Ex eodem fonte derivari poſſe cometarum quoque denſitates, oſtendere jam conatus ſum. En! ſpecimen calculi, hactenus, quod ſciam, intacti *; quod curioſis naturæ ſcrutatoribus haud ingratum fore ſpero.

In exemplum eſto cometa anni 1665, de quo Hevelius in Cometogr. p. 898. hæc ſcribit. "Die 8 "Aprilis, St. N. mane, magnitudinem capitis ex ma"culis lunaribus inveni; nucleum interiorem cum "tota materia adhærente, ſive crinibus circumfuſis, "æqualem eſſe toti inſulæ Siciliæ, hoc eſt, ſummum "6′; nucleum vero ſolum haud majorem eſſe inſula "Corſica lunari, hoc eſt, $\frac{5}{60}$ part. unius digiti luna"ris, ſive 12″ vel 13″." Cometa tunc erat in 23° 29′ ♓, cum 25° 49′ Lat. Bor. Invenio jam, per tabulas Halleianas, diſtantiam cometæ a ſole (SC) 62735; et a terra, 58441; talium partium qualium

* Ex ingenti caloris gradu quem terra arida apud cometam anni 1680 in perihelio verſantem ex radiis ſolaribus concipere poſſet, qui, Newtoni computo, quaſi 2000 vicibus major erat quam calor ferri candentis, concludit Auctor celeberrimus, quod "corpora cometarum ſunt ſolida, compacta, fixa ac durabilia "ad inſtar corporum planetarum. Nam ſi nihil aliud eſſent "quam vapores vel exhalationes terræ, ſolis et planetarum, "cometa hicce in tranſitu ſuo per viciniam ſolis tanto calore "ſtatim diſſipari debuiſſet." Princip. p. 508. Præter hoc, nihil de cometarum denſitatibus vulgatum invenio.

media telluris a ſole diſtantia ſit 100,000: et ponendo CA, latitudinem capillitii verſus ſolem, æqualem dimidio capitis, ſive 3′, ea erat 51; et AS, 62684; et ſemidiameter nuclei 1,7 earundem partium. Proinde, materia in ſole eſt ad materiam in cometa, ut 62684 × 62684 ad 51 × 51, ſive ut 1 ad $\frac{1}{1510724}$. Porro, poſita ſolis parallaxi media 8″, 68 (ut ex nupero veneris ſub ſole tranſitu collegit vir rerum aſtronomicorum peritiſſimus Jacobus Short, S. R. S.) ſemidiameter terræ eſt 4,2; ejuſque materia $\frac{1}{342686}$ pars materiæ ſolis; ut meus profert calculus. Quocirca, denſitas terræ eſt ad denſitatem cometæ, ut $\frac{1}{4,2 \text{ cub.} \times 342686}$ ad $\frac{1}{1,7 \text{cub.} \times 1510724}$; id eſt, ut 1 ad 3, 44. Hic igitur cometa, qui in perihelio ſuo quaſi decuplo propius quam terra ad ſolem accedebat, 3 $\frac{1}{2}$ fere vicibus denſitate terram ſuperabat.

Veruntamen, hos nolim venditare numeros tanquam perfectos, et cometæ hujus denſitatem abſolute exhibentes. Inſtituto meo ſufficit, ſi, dum calculi methodum illuſtrant, non longe a vero aberrent; ſummam enim accurationem ab ipſa Hevelii obſervatione haud eſſe petendam, palam eſt. Probabile omnino videtur, quòd obſervator illuſtris magnitudinem capitis æſtimârit ſecundum rectam axi caudæ normalem, quippe in ea ſola directione dimetiri potuit caput abſque cauda; et capillitii latitudo, de qua hîc agitur, nempe verſus ſolem, paulo minor fuerit quam dimidium iſtius magnitudinis. Hæc ſuppoſitio non male quadrat cum ultima obſervatione hujus cometæ, quam Hevelius habuit die 20° Aprilis. Eo tempore diſtantia cometæ a ſole, meo computo, erat 24237; et a terra, 89602. Jam, apparens latitudo capil-

capillitii verſus ſolem eſt ut diſtantia cometæ a ſole directe, per Corol. 1. Prob. V. et ut diſtantia a terra inverſe; id eſt, ut $\frac{62735}{58441}$ ad $\frac{24237}{89662}$, ſive ut 4 ad 1. Quare, ſi in priore obſervatione fuiſſet 3′, in poſteriore eſſet 45″; et capillitium emineret ſuper nucleum 40″ circiter; quod quidem non adeo ægre, adhibito perſpicillo, perceptu foret. At teſtatur Hevelius, quòd "die 20° Aprilis, cum a nobis ultimum obſer-"varetur cometa, in frontiſpicio capitis materia illa "dilutior jam adeo erat contracta, attenuata et diſſi-"pata, ut parum admodum amplius ſupereſſet; ad "utrumque latus vero ſatis dilatata extitit." Credibile igitur eſt, latitudinem anteriorem capillitii, etiam in prima obſervatione, minorem extitiſſe quam lateralem; i.e. minorem quam 3′. Quod ſi hæc latitudo minor fuerit quam 3′, minuenda erit materia cometæ, ejuſque denſitas, in duplicata ratione, quam proxime; per Corol. 2. et 3. Prob. V. Hujus ergo cometæ denſitas non major eſt quam quæ ſupra definita eſt; ſed poteſt eſſe aliquanto minor.

Quærebam itidem denſitatem cometæ anni 1682. "Aug. 20. St. V. diameter capillitii axi caudæ per "nucleum normalis, menſurante Flamſtedio, erat "2′ 0″; cujus diſtantiæ ipſe nucleus vix $\frac{1}{10}$ æquabat; "ideoque latus erat circa 12″." Hîc iterum neceſſe habemus ſumere dimidium diametri capillitii pro latitudine ejus verſus ſolem; quanquam vix dubium eſt quin hæc aliquantulo minor fuerit illo. Et in hac ſuppoſitione, iiſdem calculi veſtigiis inſiſtendo, inveni denſitatem terræ eſſe ad denſitatem cometæ ut 1 ad 0, 4562; ſeu ut 11 ad 5 circiter.

Notatu non indignam arbitror aliam hujus cometæ obſervationem a Flamſtedio factam die 4 Septemb.

quo

quo tempore erat " nucleus limbo capillitii vicinior " quam antea [20 Aug.] duplo fere." Supra monstratum est, quod capillitii latitudo apparens versus solem est ut distantia a sole directe, et a terra inverse; ex quo, computum ineundo, invenio capillitii latitudinem die 4 Septemb. esse ad ipsius latitudinem 20 Aug. ut 1 ad 2,1542. Quod cum observatis probe congruit; ut conferenti liquebit. Fatendum tamen, maximam hujus differentiæ partem secundæ illarum rationum deberi.

Vellem sane plura adducere exempla, cæterosque cometas, ratione densitatis, inter se conferre; sed observationum penuria impedimento fuit quo minus aliorum densitates ad calculum revocaverim; nullæ enim extant, quas mihi quidem videre contigit, adeo subtiles ut huic disquisitioni apte inservire queant. Nec mirum; quum admodum difficile est, arcus tam exiguos ad amussim mentiri. Quantum ex allatis conjectura assequi possum, eadem hîc valet regula quam obtinere demonstravit Newtonus in planetis, " eos nempe densiores esse, cæteris paribus, qui sunt " soli propiores *." Insuper colligere videor, quod sicut cometæ ut plurimum ejusdem fere sunt magnitudinis cum planetis, ita non multo plus discrepant cometæ a planetis quoad densitatem quam planetæ dissident inter se. Nam terra, per Newtoni demonstrata, sextuplo densior est quam saturnus. Quinetiam, quòd cùm distantiæ periheliæ sunt æquales, cometæ planetis rariores sunt; — in hunc fortasse fi-

* Secundum superiorem calculum, cometa anni 1665 erat quasi $7\frac{1}{2}$ vicibus densior quam cometa anni 1682. Eorum autem distantiæ perihe.iæ, Hallcio computante, erant 10649, et 58328.

nem,

nem, ut in transitu prope planetas, minus turbent eorum motus; et ut, eidem gradui caloris objecti, facilius vapores emittant, ad formandas caudas.

Sed manum de tabula; neque enim conjectandi licentiæ indulgendum. Si in posterum astronomi observationes hujusmodi instituerint, ea sedulitate qua phænomenis stellarum tam fixarum quam errantium invigilare solent, certiorem tandem penitioremque siderum caudatorum cognitionem sperare licebit.

P. S.

EST et infra superficiem cometæ limes attractionis, intra quem vis solis fortior est quam cometæ. Hujus investigatio pendet a diversa gravitatis lege, crescentis scil. in simplici ratione distantiæ a centro; per Prop. 73. Lib. I. Newt. Princip. ejusque natura est, quòd si a quovis ipsius puncto l (Fig. 6.) ducantur rectæ ad centrum solis S et cometæ C, quadratum prioris in posteriorem ductum, $Slq \times lC$, efficiet solidum datum; quod eandem quidem habebit rationem ad cubum semidiametri cometæ, CN, quam habet materia in sole ad materiam in cometa. Verùm, cùm hujus limitis diameter perexigua est respectu distantiæ a sole, et propterea omnes rectæ S l, ideoque et lC, sunt quam proxime æquales, hæc superficies $llll$ parum differet a sphærica; ejusque diameter erit reciproce ut quadratum distantiæ cometæ a sole. Sed hæc, aliaque ad hunc casum spectantia, Prob. II. consimilia, mathematicæ duntaxat sunt contemplationis; eoque consulto in superioribus prætermisimus, utpote ab argumento nostro aliena.

ADDENDA.

ADDENDA.

I.

Aſcenſum caudæ cometicæ, quem ope cujuſdem medii cœleſtis perſpicaciſſimus Newtonus explicatum dedit, aliquantulum illuſtrare aggreſſi ſumus. Quænam verò ſit iſtius medii natura, quiſve terminus, magna quæſtio eſt, et tenebris involuta. Diverſis nominibus illud Newtonus inſignivit, ſcilicet auræ æthereæ, materiæ cælorum, et atmoſphæræ*. Aliis in locis ſummus philoſophus aſſeruit, et variis iiſque graviſſimis rationibus aſſertum ſtabilivit, "cœlos re-"ſiſtentia deſtitui"; quo poſito, hæc aura ætherea non poteſt eſſe non rariſſima; et materia cœlorum nihil aliud quam vapores longe tenuiſſimi. Corpus ſolis ingenti atmoſphæræ pondere premi, non deſunt argumenta quæ nobis fidem faciant; et ex macularum ſolarium phænomenis certum eſt, eam, unà cum corpore ſolis, circa axem ſpatio $25\frac{1}{2}$ dierum rotari. Quòd ſi hæ maculæ triduo diutius pone ſolem latent quam ſeſe nobis conſpiciendos præbent, ut a quibuſdam obſervatum eſt, neceſſe eſt altæ ſint ſupra ſolis ſuperficiem parte $\frac{1}{137}$ † diametri ſolaris; et ad hanc minimùm altitudinem porrigi ſolis atmoſ-

* Vide periochen citatam ſub Prob. IV. in qua Auctor has voces promiſcue uſurpâſſe videtur; ſaltem non præciſe inter eas diſtinxit.

† Wolfius in Aſtron. macularum ſolarium altitudinem multo majorem juſto æſtimavit, ſcil. $\frac{3}{34}$ diametri ſolis: manifeſto errore; ponendo ſinum verſum 10 graduum æqualem 15 centeſimis partibus radii, cùm revera ſit æqualis tantùm 15 milleſimis.

phæram, par eſt credere. Cometa anni 1680 in perihelio minus diſtabat a ſole quàm parte ſexta diametri ſolis; et in vicinia illa, ex auctoris clariſſimi ſententia *, reſiſtentiam nonnullam ſentire debuit. Sed quæ ſit illius atmoſphæræ altitudo, hucuſque incompertum eſt. Veriſimile videtur, eam intra modicos terminos contineri. Etenim, ſi ad orbes planetarum pertingeret, caudæ cometicæ per eos tranſeuntes, impulſu illius gyrantis tanquam vento validiſſimo raptæ, ab occaſu in ortum detorquerentur. Atqui medium, de quo quæſtio eſt, eouſque extendi oportet, quo cauda cometæ cujuſvis pullulare incepit. Hic locus orbibus mercurii et veneris, immo et ipſius terræ †, ſuperior eſt. Tam immanis autem amplitudo atmoſphæræ ſolis concedi nequit.

Quid de re tam obſcura ſtatuendum ſit, incertus hæreo; hæc tantum rata habens, medium hocce tenuiſſimum eſſe, et quam facillime rareſcere; paulo autem denſius eſſe prope ſolem quam ulterius, terminumque ejus extra ſphæram orbis magni eſſe locandum; item, non modo non rotatum vertigine quali corpus ſolis, ſed et ſumme quietum eſſe ac tranquillum. Denique, non abſimile videtur, caudarum materiam longe magis volatilem eſſe quàm exhalationes e corporibus quibuſlibet terrenis aut planetaribus elicitas.

II.

Quibus uſibus inſerviant cometarum caudæ, id eſt alia quæſtio, explicatu difficillima. Cenſuit Newto-

* Princip. p. 525.

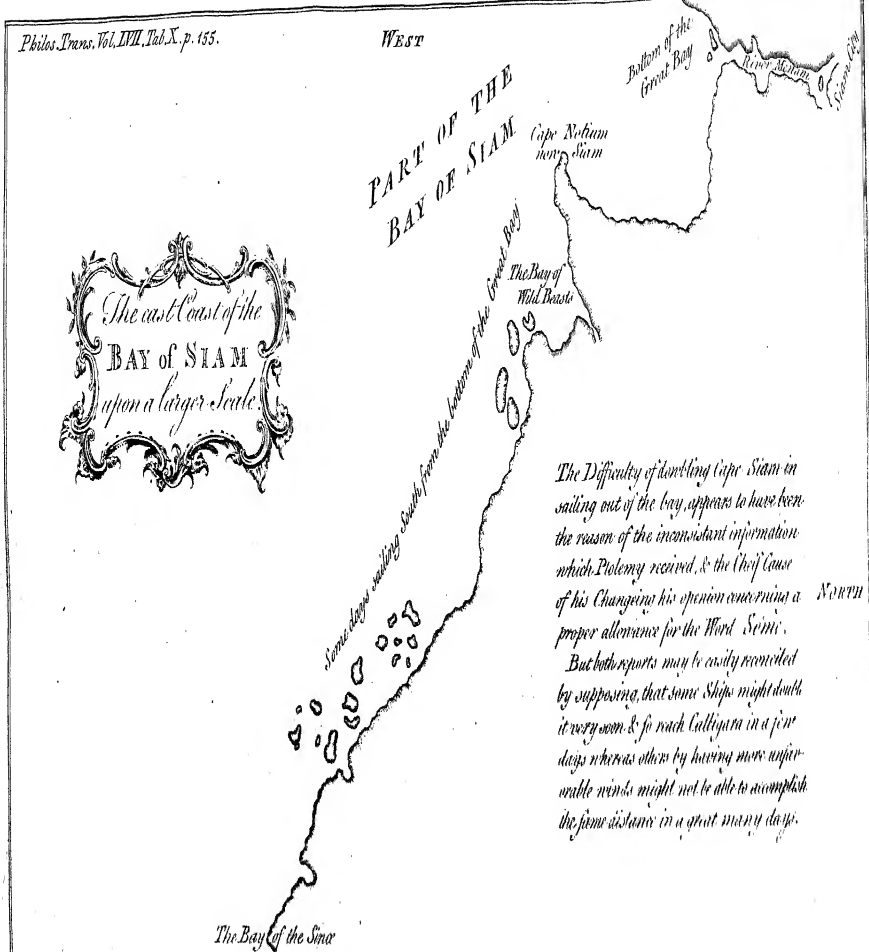
† Conſtat ex phænomenis cometæ anni 1680. Alii cometæ hunc terminum ad orbem uſque martis, aut ſupra, forſan amoveant. Sed hoc non exploravi.

us " eas * ad conſervationem marium et humorum
" in planetis requiri, ex quibus in planetas attractis
" et cum eorum atmoſphæris mixtis, quicquid li-
" quoris per vegetationem et putrefactionem conſu-
" mitur, et in terram aridam convertitur, continuo
" ſuppleri et refici poſſit." Verùm cùm, ipſo judice †, " perexigua quantitas aëris et vaporum ad om-
" nia caudarum, etiamſi ſpatia immenſa occupantium,
" phænomena ſufficiat"; et cùm hujus quantitatis perexiguæ perexigua tantum pars in unumquemque planetam incidere queat (ni mea fallat opinio in Schol. 2. Prob. IV. prolata), ambigi poteſt an hic ſit e præcipuis cometarum finibus. Sed nihil ſtatuo. Aliorum eſto judicium.

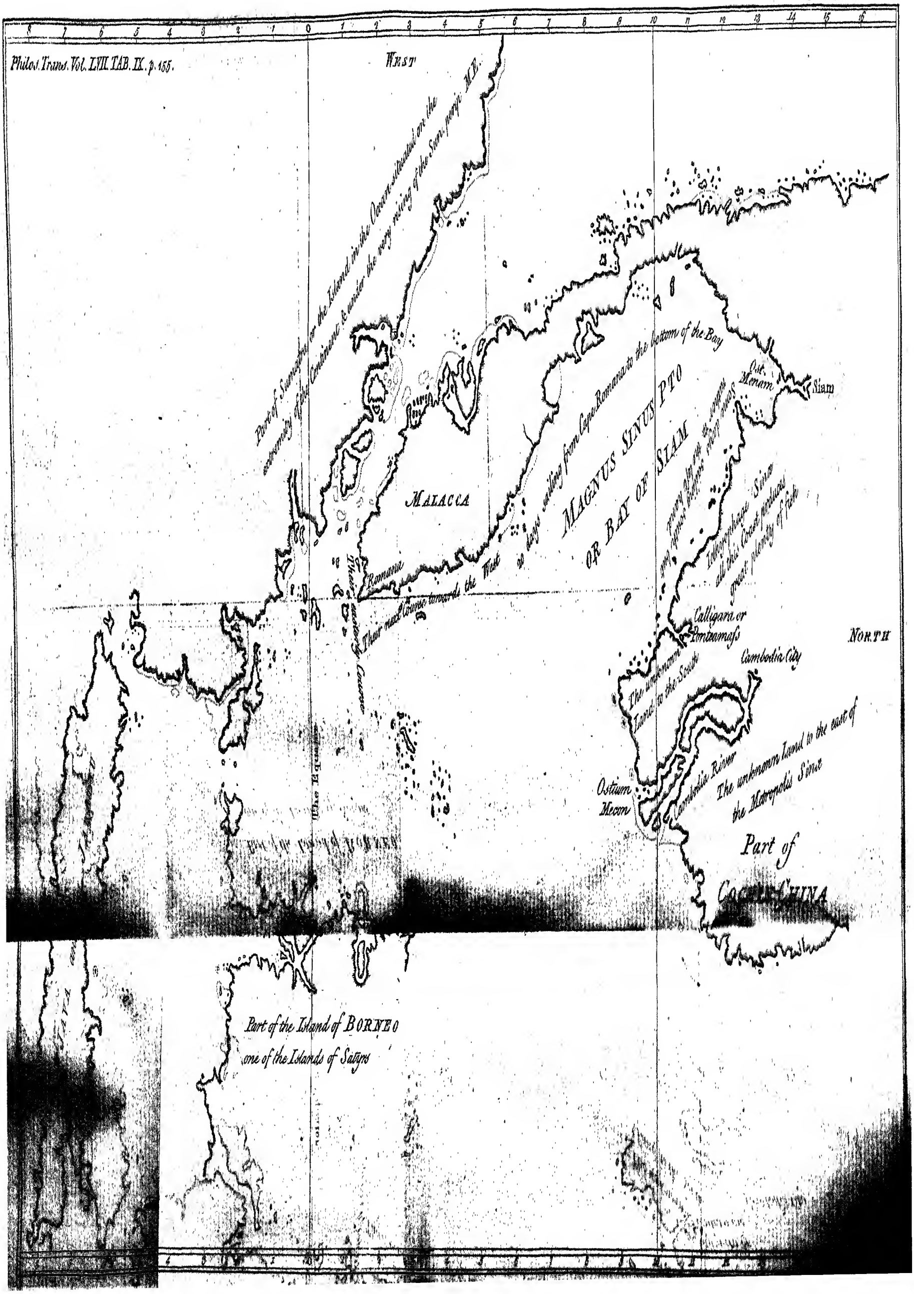
* Princip. p. 515.

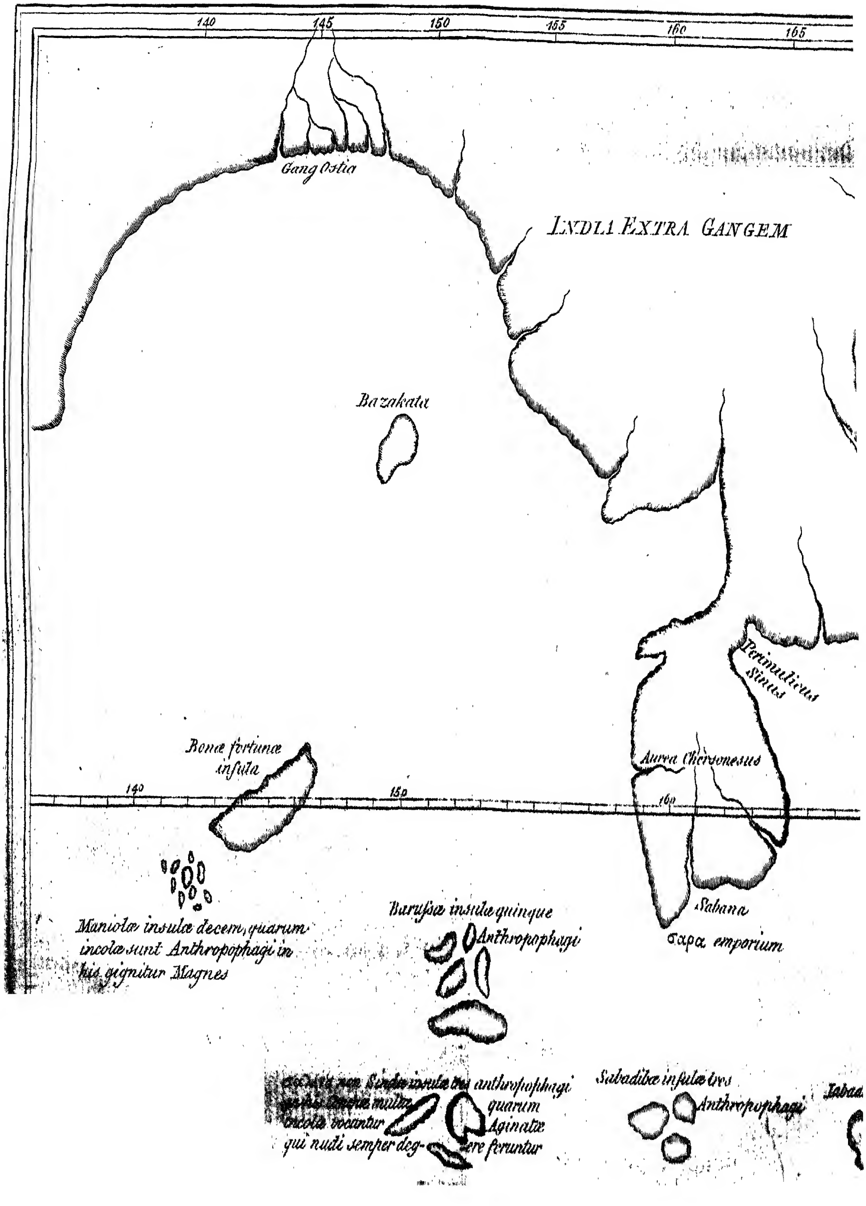
† Id. p. 513.

XVI. *Some*


Philos. Trans. Vol. LVII. Tab. X. p. 155.
WEST
PART OF THE BAY OF SIAM
Bottom of the Great Bay
River Menam
Siam City
Cape Notium now Siam
The east Coast of the BAY of SIAM upon a larger Scale.
The Bay of Wild Beasts
Some days sailing South from the bottom of the Great Bay
The Difficulty of doubling Cape Siam in sailing out of the bay, appears to have been the reason of the inconsistant information which Ptolemy received, & the Cheif Cause of his Changeing his opinion concerning a proper allowance for the Word Σινῶν.
But both reports may be easily reconciled by supposing, that some Ships might double it very soon & so reach Calligara in a few days whereas others by having more unfavorable winds might not be able to accomplish the same distance in a great many days.
NORTH
The Bay of the Sinæ
Calligara Situated upon the River Cotiaris M. Heracleot.
Also to the South
Unknown Marshy Country to the east of it.
ὅτι ταυτας ὁμολογειν ανατολικωτεραν αυτην ειναι των Κατιγαρων. P. G. L.I. c.15.
River Mecon
Cambodia City situated 3 deg.^s to the east of Calligara or 3 deg.^s of distance from it.

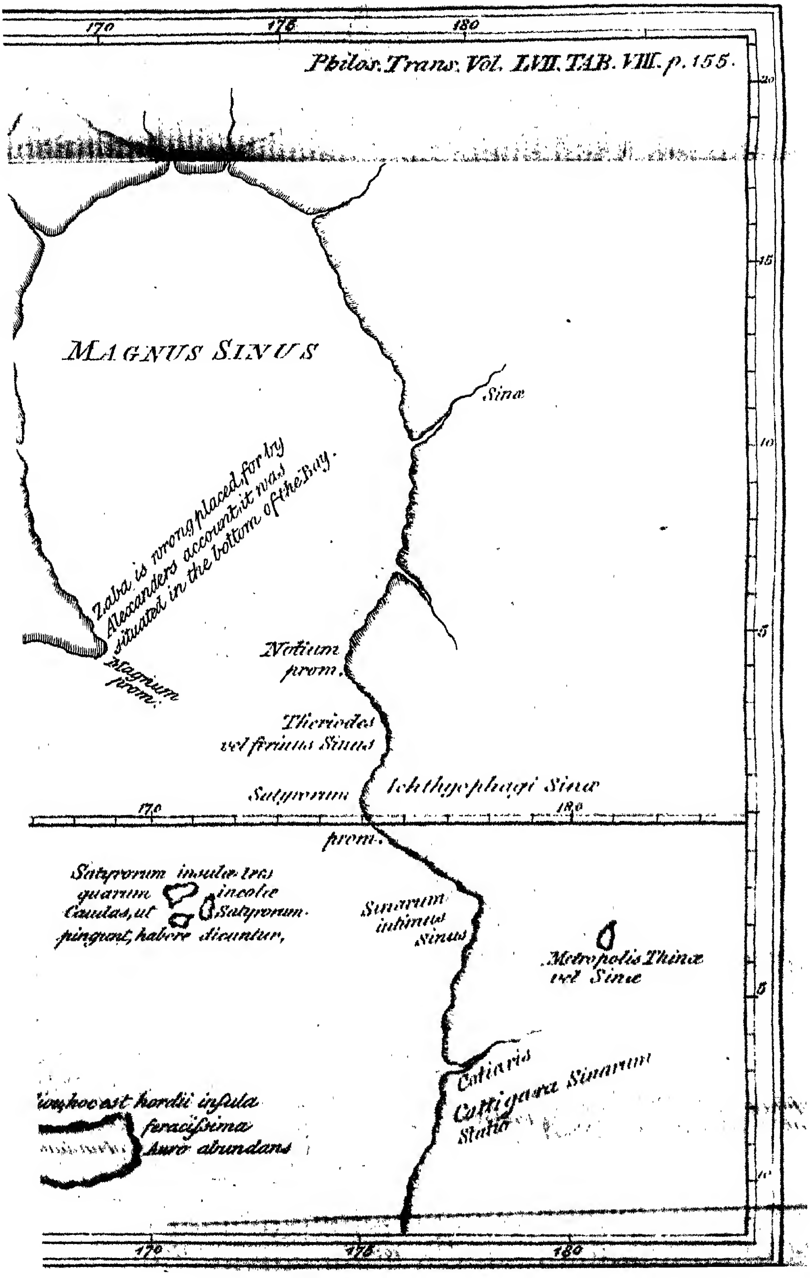
Philos. Trans. Vol. LVII. TAB. IX. p. 155.





140
145
150
155
160
165
Gang Ostia
INDIA EXTRA GANGEM
Bazakata
Perimulicus Sinus
Bonæ fortunæ insula
140
150
160
Aurea Chersonesus
Sabana
σαρα emporium
Maniolæ insulæ decem, quarum incolæ sunt Anthropophagi in his gignitur Magnes
Barussæ insulæ quinque Anthropophagi
anthropophagi quarum incolæ vocantur Aginatæ qui nudi semper degere feruntur
Sabadibæ insulæ tres Anthropophagi

Philos. Trans. Vol. LVII. TAB. VIII. p. 155.



Received February 6, 1767.

XVII. *Some Attempts to ascertain the utmost Extent of the Knowledge of the Ancients in the* East Indies: *by* Mr. John Caverhill.

Read March 19, 1767.

ALL the moderns who have treated of the geography of the ancients, more particularly in their account of the Sinæ, the most oriental country they were acquainted with, have differed so widely from the only guides they had to follow to the east of the Ganges, that I have undertaken to lay before the Royal society some observations, which may contribute to determine, with greater precision, the situation and limits of this country.

But as geographers have been unanimous in supposing the Aurea Chersonesus to have been the same as the present kingdom of Malacca; the only difficulty, which remains, is to discover how far they sailed beyond it, in the days of Ptolemy. And, that the proofs alledged in support of what may be asserted in the course of this disquisition may be better understood; I have thought it necessary to annex a correct modern chart of this Peninsula, and have also added the outlines of Ptolemy's, (See Tab. VIII. IX. X.) for reasons which will soon appear.

It may be proper first to observe, that Cattigara was the name of a port situated somewhere beyond the

the Aurea Cherfonefus or Malacca; and that the antients had never failed farther than Cattigara: for contiguous to it was a terra incognita [a]. But at what diftance the Aurea Cherfonefus was from Cattigara, Ptolemy himfelf was ignorant; for he fays, " that Marinus, who is quoted by him upon " this occafion, had not marked the number of the " ftadia: τȣ δε απο της χρυσης Χερσονησȣ επι τα Κατ- " τιγαρα διαπλȣ τον ϛαδιασμον ὁ Μαρινους ουκ εκτιθεται [b]." " It was faid however that Alexander reported the " land from thence lay upon the north fide of the " line; and that thofe who failed along the fhore, " arrived at the city of Zaba in twenty days; from " Zaba failing fouth, and efpecially to the left hand, " in fome days they reached Cattigara: φησι δε " Αλεξανδρον αναγεγραφεναι την γην εντευθεν εναντιαν ειναι " τη μεσημβρια, και τους πλεοντας παρ αυτην εν ἡμεραις " εικοσι καταλαμβανειν πολιν Ζαβας. απο δε των Ζαβων " προς νοτου διαπλευσαντας, και μαλλον εις τα ευωνυμα " ἡμερας τινας, ενδεχεσθαι τα Καττιγαρα." In order therefore to endeavour to acquire fome knowledge of the diftance, Ptolemy begins by comparing it with the fame number of days failing on a part of the coaft of Africa, with which he was acquainted; and, after this uncertain calculation, concludes, that thefe two places might be diftant from each other 17 degrees and $\frac{1}{6}$.

As his only view through this whole chapter is to finifh what he had begun in the foregoing ones, the longitude of the then known world, his errors are the more excufable; efpecially as his calcula-

[a] Ptol. Geograph. l. i. c. 17. Paris, 1546. [b] Lib. i. c. 14.

7 tions

tions will be found very near the truth: his greatest miſtake therefore is in adding theſe 17 degrees to the longitude of the earth; whereas Cattigara was to the latitude of Malacca, and almoſt upon the ſame Meridian.

Although Ptolemy no where particularly mentions from what part of Malacca the ſailors who went this voyage took their departure, yet as his moſt ſouthern point of land in this Peninſula, is exactly 17 degrees of longitude from Cattigara, and as Romana is the moſt ſouthern cape of Malacca at preſent, this clearly indicates it to have been near that cape, but to the weſt of it.

For elſewhere he acquaints us [c], "that he had "learned many particulars concerning the inland "parts of the provinces and countries of India, even "to the Aurea Cherſoneſus, and from thence to Cat-"tigara, and the ſailors who went this voyage agreed "that it was to the eaſt; but that they immediately "returned towards the weſt; and that the time "for performing this laſt voyage was irregular, and "not fixed: παρ ὧν και τα τε αλλα περι την Ινδικην "μερικωτερον, και κατα τας επαρχιας, εμαθομεν· και ταυτας "της χωρας ενδοτερω, μεχρι της χρυσης Χερσονησου, και εν-"τευθεν ἑως των Καττιγαρων. το μεν ὁτι προς ανατολας εστιν, "ὁ πλους εισπλεοντων, και παλιν εξιοντων προς δυσμας συν-"ιστορουντων. του δε ατακτον και ανωμαλον τον χρονον των δια-"νυσεων προσομολογουντων."

To explain this paſſage it may be obſerved, that their ſailing firſt to the eaſt was from neceſſity, becauſe they had not as yet doubled cape Romana, the

[c] Lib. i. c. 17.

moſt

moſt ſouthern point of land in Aſia, from whence they were obliged, by their own rules of ſailing, to return towards the weſt, after having doubled the cape. This does not appear ſo weſterly by the chart as it did to them; becauſe a ſtrong eaſterly tide ſets by the cape, which, by carrying them too far eaſt, would make their next courſe toward the weſt, and which would ſoon become intirely ſo by the figure of the land [d].

Alexander reported, that the land lay north of the line; they had imagined cape Romana, as well as ſome other parts of Malacca, lay ſouth of it; they were therefore, no doubt, ſurprized to find the land retreat.

He has told us that they arrived at Zaba in 20 days, and from it ſailing ſouth, but rather to the left hand, in ſome days they reached Cattigara. Zaba therefore ſtood ſomewhere upon the bottom of the bay of Siam; becauſe no other ſituation but the bottom of the bay of Siam obliged them to alter their courſe to the ſouth; and including the inequality of the coaſt, it will be allowing a ſufficient diſtance for the number of days they were in ſailing it.

From Zaba they began to ſail ſouth, and immediately follows, rather to the left hand; now they could not ſail ſouth and incline to the left hand without approaching the eaſt; this therefore might be any point between the ſouth and eaſt. The figure of the coaſt runs exactly in this direction, and is a proof of the accuracy of the deſcription.

[d] Nicholſon's Obſerv. upon Eaſt India Voyages.

In this courſe they continued ſome days before they reached Cattigara. Ptolemy juſtly finds fault with the uncertainty of the expreſſion, *ſome* days, and is at a loſs how many days to allow for *ſome*; but, as he had no better information, and as they arrived at Cattigara by obſerving this ſouth eaſt courſe, it muſt have ſtood ſomewhere to the north weſt of the mouth of the river Cambodia.

Marcianus Heracleota alſo expreſsly mentions that they which ſailed to Cattigara held the ſame courſe. His words are, "ſailing therefore out of the great
" bay, and the promontory Notium, as if to the
" ſouth, and keeping the country of Sinæ to the left
" and to the eaſt; you meet with a bay pertaining
" to wild beaſts, which reaches to the promontory
" of Satyrs; that after this promontory follows the
" greateſt bay called the bay of Sinæ, which extends
" to the Terra Auſtralis et Orientalis incognita, into
" which bay runs the river Cotiarin, and upon which
" river ſtands Cattigara a ſea-port of the Sinæ: Πλε-
" οντι τοινυν μετα τον μεγαν κολπον, και το Νοτιον ακρον, ὡς
" προς την μεσημβριαν, και αριστερον εχοντι την τε των
" Σινων γην, και την ανατολην· εκδεχεται κολπος θηριωδης
" καλουμενος, διηκων μεχρι του ακρωτηριου του καλουμενου
" Σατυρων ακρου. Απο δε του ακρου των Σατυρων, εκδεχεται
" κολπος μεγιστος, καλουμενος Σινων κολπος. παρηκει δε οὗτος
" μεχρι της μεσημβρινης αγνωστου γης, ἡ συναπτει και της
" ανατολης αγνωστον γην. απο δε του Κοτιαρου ποταμου, εκδε-
" χεται Καττιγαρα ὅρος μεν Σινων."

For theſe reaſons it is extremely probable that Cattigara ſtood ſomewhere upon the north eaſt coaſt of the bay of Siam. A very intelligent modern

dern Navigator* gives the following description of it: "For 50 leagues from Liam point it is a barren "sandy desert to Ponteamass, by far the most considerable port on the coast, and a place of pretty good "trade for many years, but a narrow river, which "in the rainy seasons of the south west Monsoons "has communication with Bansac or Cambodia river, "which made it draw foreign commerce from the "city of Cambodia hither, for the city lieth near "100 miles up the river, and most part of the way "a continual stream, made the navigation so trou-"blesome as few cared to trade to it, for which rea-"son they came to Ponteamass."

As this therefore is by far the most considerable port on the coast, we may reasonably suppose it to be the same with the antient Cattigara. The following arguments will serve to confirm the validity of this supposition.

The distance between Ponteamass and the bottom of the bay, where they began sailing for some days south, may very well agree with the 4 degrees Ptolemy allows for it. It may be remembered that he had supposed the 17 degrees beyond Malacca, were situated to the east of it; this supposition which has before been found to be erroneous, Ptolemy seems now to reason from as a fact, in order to support his former hypothesis, notwithstanding his declaration that he thought it ridiculous to imagine, as others had, that τινας ἡμερας should signify many days. We have already seen that his first error consisted in imagining that Cattigara lay 17 degrees east

* Hamilton's Account of the East Indies, vol. ii.

of

of cape Romana, whereas it was nearly to the weſt of it. The ſecond error was in protracting the eaſt ſide of the bay of Siam 18 degrees farther than the land at preſent exiſts. And it will immediately appear in what manner this happened.

The boundaries of the Sinæ to the weſt, is in his 173 degree of longitude, and the moſt ſouthern end of that parallel, in the 17th of north latitude [f]. Cattigara in the 177th and 8th of ſouth latitude [g]. This difference was the whole maritime extent of the country. In his deſcription of the weſt ſide of the bay, he had conſumed 13 degrees of longitude, that is from cape Romana to the bottom where the land began to reflect ſouth; and theſe are what he allowed for the 20 days ſailing already mentioned; there were only 4 degrees remaining between the bottom of the bay and Cattigara; but, as that would not at all agree with the accounts he had received of its being a great bay, when one ſide was ſo much ſhorter than the other, he might think his firſt explanation of the word *ſome*, erroneous; more eſpecially as the other ſignification that was given, which was to take it for *many*, even ſo *many*, that they could not be numbered, would intirely coincide with his firſt

[f] Lib. vii. cap. 2.

[g] Lib. vii. cap. 3. It has been a prevailing opinion that Agathemerus made theſe tables we have annexed to Ptolemy's geography, but this appears only to have been done in conſequence of the longitudes and latitudes Ptolemy had ſettled. For when we ſee him fall into ſo great an error in his firſt book, which none doubts to be his own, the adding theſe ſuppoſititious 17 degrees to the longitude of the earth; why may we not with as much appearance of juſtice lay this ſecond fault to his charge, eſpecially as we have no proofs to the contrary?

Theory. Accordingly he has put down 25 degrees of latitude for *ſome*; but in ſuch a manner that the ſouth end of this imaginary line was not more than 4 degrees eaſt of the north end in the bottom of the bay of Siam; by which he has not only contradicted that very Alexander he himſelf quotes, who told him the land was to the north of the line; but renounced his firſt rational opinion, in thinking four degrees might be a ſufficient allowance for *τινας*.

2d. The country beyond Ponteamaſs exactly agrees with Ptolemy's deſcription of that beyond Cattigara, "a marſhy country, which produced reeds of ſuch "a ſize, that when they were joined and tied together, they were enabled to paſs from one ſide to "the other. *και τα αναλολικωλερα ταυλων, αλνωςος εςι γη, "λιμνας εχουσα ιλευωδεις, εν αἷς καλαμοι μεΓαλοι φυοιλαι, "και συνεχεις οὕτως, ὡς τε εχομενους αυλων, ποιεισθαι τας "διαπεραιωσεις* [h]." Now the kingdom of Cambodia is annually overflowed with water during the ſouth weſt monſoons, the very ſeaſon in which the ſhips arrived there; ſo muſt have exactly anſwered theſe appearances and his deſcription.

"Cambodia, or Camboxa, is annually overflowed "by the river Menam, one of the largeſt in India, "carrying ſo much water that it floods and covers "the fields, like the Nile in Ægypt; for ſix months "it runs backwards. The reaſon of it is the extent "and plainneſs of the country it runs along, and the "ſouthern breezes which choak up the bar with "ſand [i]."

[h] Lib. i. c. 17.

[i] Argenſola, Hiſtory of the Spice Iſlands.

3d.

3d. No other country will correspond with his description of the Sinæ; "they were bounded on the "north by part of Serica[k], on the east and south by "an unknown land, on the west by India, without "the Ganges, according to the parallel already men- "tioned, and the great bay, and by the parts adja- "cent of the bay inhabited by wild beasts, and a part "of the bay of Sinæ inhabited by Æthiopian fish- "eaters: οἱ Σιναι περιοριζονται, απο μεν αρκτων τῳ εκτε- "θειμενῳ μερει της Σηρικης, απο δε ανατολων και μεσημβριας "αγνωστῳ γῃ. απο δε δυσεως, τῃ εκτος Γαγγου Ινδικῃ, κατα "την διωρισμενην μεχρι του μεγαλου κολπου γραμμην, και "αυτῳ τῳ μεγαλῳ κολπῳ, και τοις εφεξης αυτῳ κειμενοις, "τῳ τε καλουμενῳ Θηριωδει, και τῳ των Σινων, ου περιοι- "κουσιν ιχθυοφαγοι Αιθιοπες[l]."

We have already spoken of this unknown land to the east; but the land was unknown to the south likewise, not only according to this description of Ptolemy, but by a passage of Marcianus Heracleota, who lived after him, and had such other information as

[k] Ptolemy has placed this nation too far east, as is evident from a passage both in Dionysius and Rufus Festus Avienus. He has made an imaginary parallel intersect the 180th degree of longitude, and run north to the 63d degree of latitude, and bound both these nations to the east; and it would be difficult to conjecture the cause of this mistake, had these not been the most oriental nations he was acquainted with. This error appears to have bewildered all the moderns, who have attempted to ascertain the situation of this country. But as we hope to be able to prove that the present kingdom of Cambodia was the Sina of Ptolemy; the country of the Seres, by that rule, would be part of Thibet, and north of it to the 63d degree of latitude. But their true situation appears to have been in some part of the present Buckaria, to the east and north east of Samarcand.

[l] Lib. vii. c. 3.

disco-

discoveries produced during the interval from Ptolemy's death. He says, " we ought to conceive two
" unknown lands, one extending to the east, which
" the Sinæ possess, and the other towards the south,
" which stretches through the whole Indian ocean,
" so that both these unknown lands meeting, form
" as it were a certain angle in the bay of the Sinæ:
" δυο γαρ αγνωστους ὑπονοειν χρη γας, την τε παρα την ανα-
" τολην διηκουσαν, ἡ παροικεν ειρηκαμεν τους Σινας, και την
" παρα την μεσημβριαν, ἥτις διηκει παρα πασαν την Ινδι-
" κην θαλασσαν· ὡστε συναπτουσας, ἑκατερας τας αγνω-
" στους γας, καθαπερ τινα γωνιαν αποτελειν περι τον των
" Σινων κολπον[m]." Now Cattigara stood upon a river which ran into this bay; and as they had never sailed so far as the mouth of the river Menam, the accuracy of this description is very apparent.

This country was bounded on the west by India, without the Ganges; or, as he has said elsewhere, by the 173d degree of longitude; extending north from the middle of the great bay to the country of the Seres, and the great bay, and parts adjacent of the bay of wild beasts, and part of the bay of the Sinæ, inhabited by Æthiopian fish-eaters[n]. These two bays were, by this description, evidently on the east side of the great bay, and between Cattigara and the bottom, or between the present Ponteamass and the mouth of the river Mecon.

For the same reasons therefore that the present Ponteamass seems to have been the ancient Cattiga-

[m] M. Heracleot. p. 29.

[n] Captain Hamilton says that the rivers of this country abound with many kinds of fish, which are a considerable object of trade among the inhabitants.

ra:

ra; the modern city of Cambodia will be the antient metropolis Sinarum.

Ptolemy tell us, "that Cattigara lay south west "of the metropolis: η δε απο της μετροπολεως των Σινων "επι τον ορμον, τα Κατιγαρα, προς δυσμους εςι, και με-"σημβριαν.[o]" So does Ponteamass from the city of Cambodia.

He agrees with the author of the Periplus of the Red sea, "that this metropolis was a Mediterranean "city, although he says it had no brazen walls, nor "any thing worthy of notice: ουτε μεντοι χαλκα τειχη "φασιν αυτην εχειν, ουτε αλλο τι αξιολογον[p]."

The author of the Periplus says, "that beyond "the Ganges, upon the eastern extremity of this "country, under the very rising of the sun, there is "an island in the ocean, having most excellent tor-"toise shell, and all things that are to be found about "the Red sea. And that after this country, imme-"diately without it, in some place where the sea "ends, is situated the greatest Mediterranean city, "called Thina: κατ' αυτον δε τον ποταμον νησος εςιν ωκεαν-"ειος, εσχατη των προς ανατολην μερων της οικουμενης, υπ' "αυτον ανεχοντα τον ηλιον, καλουμενη χρυση, χελωνην εχουσα "παντων των κατα την Ερυθραν τοπων αριςην. μετα δε ταυ-"την την χωραν, υπ' αυτον ηδη τον βορεαν εξωθεν εις Σινων τινα "τοπον, αποληγουσης της θαλασσης, παρακειται εν αυτη "πολις μεσογειος μεγιςη λεγομενη Θινα[q]."

This island in the ocean, which stood upon the extremity of this country, appears to be the present

[o] Lib. i. c. 17.
[p] L. vii. c. 3.
[q] Peripl. M. E. p. 36. Geog. Veter. Script. Oxon.

island of Sumatra, which fronts both the most southern and eastern parts of Malacca.

He has not in the least confounded this peninsula with the island, notwithstanding he calls them both by the name of Aurea; the first was upon the extremity of the east; the island also was upon the extremity of the east, but it was likewise under the very rising of the sun. Had he not been acquainted with this distinction, he would not have used the word χωραν, but νησον (so it would have been), after this island, but we see that he says it was, after this country, and immediately without it, in some place where the sea ended, where this city was situated.

This exactly agrees with the bay of Siam; it lies after Malacca, for we must pass that peninsula before we can arrive at it; it is also immediately without it, and towards the north in some place where the sea ends. The bottom of the bay of Siam is 13 degrees north of cape Romana, and there the sea may be said, without much impropriety, to end; more especially as in that place was situated this city, beyond which we have no accounts of any further investigations made in his days.

This very concise but accurate narrative, at the same time that it gives all the proof we can possibly expect that Sumatra was not at that time joined to the continent, so likewise does it demonstrate this metropolis to have been situated somewhere in this bay; and although we might conjecture, with as great an appearance of probability from these circumstances alone, that it was Siam, rather than Cambodia; yet from the collateral evidence already produced, that they sailed out of the bay before they arrived at its port, it is evident it lay south east of Siam,

 and

and the teſtimony of Ptolemy and Marcianus Heracleota, who aſſert that the ſame ſouth eaſt courſe was held in order to reach Cattigara, being added, it neceſſarily follows, that that port was ſituated to the weſt of that cape which faces Puli Ubi [r]; but as there is no city weſt of that cape, on this particular part of the continent, except Cambodia, we may therefore reaſonably ſuppoſe that city the antient metropolis Sinæ [s].

It may be obſerved that the ancient city of Thina and modern city of Cambodie not only agree in ſituation, but alſo in the nature of their produce. Formerly " they exported fine otton [t] andflo wered

[r] See Plate IX.

[s] Voſſius wondered how any one could doubt that the antient Sina was the ſame with the modern Siam, without giving any reaſons at all for his conjecture. " Quis dubitare poſſit, quin illa " ſit ea ipſa, quæ nunc Siam appellatur?" Voſſ. Obſ. ad P. Melam, p. 560. Lugd. B. 1748. Whether or not Cambodia had the ſame name formerly with Siam, or was a province to it, is at this time impoſſible to determine; but it manifeſtly appears, that this metropolis was neither the preſent city of Siam, nor ſituated in any part of the country now known by that name.

[t] Οθονιον το σηρικον ſeems to have been rather a vegetable, than vermicular production; for, although it may ſignify any ſpecies of web, it more properly denotes ſuch as are made of cotton or lint; on that preſumption, we have tranſlated it flowere dmuſlins; for, as theſe webs were made in the manner of thoſe by a people who were called the Seres, it appears by the following lines that the figure of flowers were interwoven with theirs:

—— εθνεα βαρβαρα Σηρων,
Εἱματα τευχουσιν πολυδαιδαλα, τιμηεντα,
Ειδομενα χροιῃ λειμωνιδος ανθεσι ποιης.
" gentes barbara Serum
" Veſtes faciunt, varii artificii, pretioſas
" Similes colore pratenſis floribus herbæ."
Dionyſ. Perieg. ver. 752.

" muſlins

" muslins by land to the coast of Malabar and Guga-
" rati: αφ' ἧς τοτε εριον, και το οθονιον το σηρικον, εις του
" Βαρυγαζαν δια Βακτρων πεζη φερεται, και εις την λιμυ-
" ρικην πολιν, δια του Γαγγου ποταμου[u]. At present
" they have cotton manufactures of various sorts,
" white and painted callicoes, muslins, buckrams,
" dimities, carpets, and silk tapestry, with other cu-
" rious pieces, finer than any from the manufactories

——" & plurima millia Serum
" Illis nulla boum, pecoris, nec pascua curæ,
" Vestibus utuntur texunt quas floribus ipsi."
Perieg. Priscian.

These goods must have been more excellent than any which India, west of that city, at that time produced, or otherwise they would never have gone so far to seek them; from which it may be suspected that this invention of fabricating such beautiful muslins was exported with these merchandises, and is of much later date in Bengal than in this country to the east of it. Cambodia appears to have been called Rachmi by two Arabian travellers, who went to China in the ninth century; and even at that time the finest muslins in the world were manufactured by its inhabitants. In this same country, say the travellers, they make cotton garments in so extraordinary a manner that no where else are the like to be seen; these garments were for the most part round, and wove to that degree of fineness that they may be drawn through a ring of a middling size. M. Renaudot, Translat. 1733. Lond.

We have supposed that those kingdoms were the same, because Rachmi was contiguous to a kingdom seated upon a promontory, which appears to have been either the kingdom of Ava or Siam, to which the peninsula of Malacca was formerly a province; likewise there was but one kingdom between it and China, and north of it lay a country called Kaschin, which, by their description, appears to have been the kingdom of Laos. This will still be more probable when we consider that no country immediately to the east or west of it, viz. Pegu, Siam, or Cochin-China, are famed for these manufactories.

[u] P. M. E. ubi supra.

" in

"in Holland; besides, the inhabitants are the most "subtle merchants in the east, and the country itself "is superiour in fertility to most of those which "surround it[x]."

We are told "that this city was difficult of access, "and that but a few people were acquainted with "the course in this voyage, and that these few seldom "sailed so far: εις δε την Θινα ταυτην ουκ εστιν ευχερως απ- "ελθειν. σπανιως γαρ απ' αυτης τινες ου πολλοι ερχον- "ται[y]," it may be imagined that the great distance of this city, and the irregularity of the winds in the bay of Siam, which happens in all bays, was the occasion of substituting these caravans in preference to the voyage; more especially as these two articles, which are mentioned to have been exported, were extremely light and portable, and could be easily transported by land to these countries already mentioned, where they would be bought up, and dispersed over Europe by the merchants of Ægypt.

But, notwithstanding we are ignorant of the time when the route of these caravans was established, yet we have no reason to presume, as an ingenious gentleman has[z], that this had taken place in the time of any of the Ptolemies; when no author, before the emperor Trajan, has so much as mentioned this nation. Indeed it is most probable this country was discovered by such ships as that great prince sent to India, with a design of acquiring what information they could receive; in order, by their intelligence, to be as-

[x] Argensol.
[y] P. M. Eryth.
[z] Schmidt, Opuscul. p. 184.

 sisted

ſiſted in the deſigns he had upon that country, after his conqueſt of Arabia [a].

There are remains of the priſtine grandeur of Cambaia [b]; and its being famous in almoſt the ſame kind of produce is a ſtrong indication of its obligations to the commerce of Cambodia. The marble ruins of an extenſive city have lately been diſcovered [c] to the north weſt of Cambodia, and to all appearance in the very route of theſe caravans; but on this occaſion, as on many others, we are but too ſenſible of the deficiency of intelligence, and of the great havock of time, which has involved the tranſactions of this period in almoſt impenetrable obſcurity.

At the ſame time, that the antients extended their knowledge upon the continent, they muſt unavoidably have been acquainted with ſuch of theſe iſlands as were moſt contiguous to it. Accordingly Ptolemy [d] has given us the names of ſeveral, in progreſſion from the Ganges to Sumatra, which he has called, with two others he has joined to it, Σαβαδειβαι; this ſuppoſition will appear more probable, as theſe iſlands are in the ſame longitude with Malacca, and directly ſouth of that peninſula.

When they were upon that part of the coaſt which faces Malacca, they appear to have proceeded along the remaining part of the north eaſt ſide of it, as far as Java, which he has named Jabadiu, Ιαβαδιου [e].

[a] Trajan died in the 118th of the Chriſtian era.

[b] Hamilton.

[c] Argenſol.

[d] Ptol. G. l. vii. c. 2.

[e] Jabadiou ſhould perhaps be rendered Javadiv, as in the Malayan language Dib or Dive ſignifies an iſland, and Giava barley, which this iſland, according to Ptolemy, produced in great quantities. As Giava is derived from an old Perſian word of the

And

And the inhabitants of this iſland were more civilized than ſome of the neighbouring ones, who were all cannibals, " for it had a ſilver metropolis, and " produced not only gold but plenty of barley: ὁ σημαινει κριθος νησος. ευφορωτατη δε λεγεται ἡ νησος ειναι, " και ετι πλειστον χρυσον ποιειν. εχειν δε μετροπολιν ονομα " αργυρον."

Next to it were the three iſlands of Satyrs; " they " were called ſo becauſe the men who inhabited " them, were ſaid to have ſuch tails as the Satyrs " were uſually painted with; ταυτας [f] οἱ κατεχοντες, " ουρας εχειν λεγονται, ὁποιας διαγραφουσι τας των σατυρων." Theſe were moſt probably the Celebes [g], Borneo, &c. " for immediately after them were ſaid to be other " ten iſlands (called Manillas), which produced ſuch " large quantities of loadſtone, as have been ſaid to " attract ſhips aſhore, which happened to be con- " ſtructed with iron nails, for which reaſon the in- " habitants made uſe of wooden ones: φερονται δε και " αλλα συνεχεις δεκα, εν αἱς φασι τα σιδηρους εχοντα ἡλους " πλοια κατεχεσθαι, κατα της ἡρακλειας λιθου περι αυτας " γεννωμενης. και δια τουτο επιξυλοις ναυπηγεισθαι, κατε- " χειν τε και αυτας ανθρωποφαγους, καλουμενους Μανιο- " λας [h]."

ſame ſignification, it would appear that both the name of the iſland and that of the grain were exported from that country. Hyde's, Hiſtor. Relig. Vet. Perſ. &c.

[f] Ibidem.

[g] The Celebes is infeſted with numbers of miſchievous and dangerous great monkeys, which keep in bodies too hard for any wild beaſts to hurt them, and are only afraid of ſerpents, which purſue them to the very tops of the trees, and devour them. Bowen's Geogr. Dict. v. ii. p. 378.

[h] The Manillas are original names, and were called ſo by the inhabitants before the Spaniards took poſſeſſion of them. Argenſol, Hiſtor.

Notwithſtanding Ptolemy has mentioned the Philippines; yet we don't imagine that any of the perſons from whom he acquired his information had ever been there: but that they had heard of theſe places at Java (to which they might eaſily have ſailed), either from the Javaneſe themſelves, or from the inhabitants of the circumjacent iſlands, who reſorted to Java for the ſame advantages of commerce which they themſelves came in purſuit of [i].

However, although they muſt almoſt neceſſarily have been acquainted with Sumatra, yet it is evident they had never ſailed quite round it; for, if they had, they would certainly not have mentioned Ceylon as the largeſt iſland in the ocean [k].

From hence it would appear that they only knew part of Sumatra and Java; and either conjectured theſe were iſlands, or depended upon ſome informations they might probably have received from the inhabitants of theſe places, relative to this particular.

So that here we may venture to fix the limits of Ptolemy's knowledge; for, as theſe iſlands at that time were but a late diſcovery, they were very imperfectly known; and unfortunately the geographers, who lived after him, were all ſo prepoſſeſſed with his ſuperior abilities, that they imagined his accuracy would bear no correction, and that he had exhauſted the ſubject. For no other author mentions any diſcoveries to the eaſt of theſe, taken notice of by him;

[i] The diſtance between Malacca and Java was leſs than between Malacca and Cattigara; beſides, the ſouth weſt monſoon was a fairer wind to the firſt than the laſt place, and of conſequence this voyage muſt have been much ſhorter.

[k] Agathem. l. ii. c. 8.

and Marcianus Heracleota had ſuch an opinion of his great merit, as to call him by the name " of the " moſt divine and moſt wiſe Ptolemy; εκ τας γεωγρα- " φιας του θειοτατου και σοφωτατου Πτολεμαιου."

By a retroſpect on ſuch authors as have been quoted, and ſome others who wrote nearly at the ſame time, according to the order in which they lived, this ſubject will ſtill appear in a clearer light.

In the days of Strabo, who lived before the Chriſtian æra, and is ſuppoſed[l] to have ſurvived it 28 years, few people had ſailed ſo far as the Ganges, " σπανιοι μεν και πεπλευκασι μεχρι του Γαγγου[m];" and being intimately acquainted with Gallus, who was the third governor of Ægypt, he had undoubtedly the moſt favourable opportunities of the moſt authentic intelligence concerning naval affairs.

Pomponius Mela is ſuppoſed to have writ before Pliny[n], in the reign of Claudius, and 30 years after Strabo. In that interval, there appears to have been made ſome farther diſcoveries upon the continent[o] to the eaſt of the Ganges; but ſo very imperfect, that they either imagined that country was an iſland, or had confounded their deſcriptions of it with theſe iſlands, which they would neceſſarily meet with in this voyage. For it is very certain, from Mela's own words, that his knowledge of theſe places we are ſpeaking of was extreamly obſcure, as all he has ſaid

[l] Voſſius.

[m] Strab. l. 15.

[n] Voſſius, Bayle's Hiſt. Dict.

[o] Strabo had probably the ſame information; but, as it was ſo extremely imperfect, did not think proper to mention it in his geography of this country.

"que

of them is, "ad Tabim insula est Chrysa, ad Gangem Argyra, altera aurea soli, altera argentea; atque ut maxime videtur, aut ex re nomen, aut ex vocabulo ficta fabula est."

The elder Pliny died in the 79th of the Christian æra, and was a cotemporary of Mela; and seems to have referred to the above passage, in the following words: "Extra ostium Indi Chryse et Argyre, fertiles metallis, ut credo; nam quod aliqui tradidere, aureum argenteumque iis solum esse, haud facile crediderim [p]."

Although the age in which Solinus lived is so uncertain, yet it might be imagined that it was not very long after Pliny; having copied from the other geographers which went before him, he has advanced nothing upon this point that had not been already mentioned. His words are these: "Extra Indi ostium insulæ duæ, Chryse et Argyre, adeo fœcundæ copia metallorum, ut plerique eas aurea sola prodiderint habere et argentea [q]."

Josephus was 56 years of age, in the fourteenth year of Domitian's reign, or 93d of the Christian æra [r]; and appears to have had a little more knowledge of these places than any we have yet mentioned; for, speaking of Saphira, from whence king Solomon had his gold, he says, that "it was a country of India, and not an island; and that it was now called by the name of Aurea, νυν δε Χρυσην γην καλουμενην, της Ινδικης εστιν αυτη."

[p] J. Harduin, Plin. p. 322. vol. I. Paris, 1723.
[q] C. J. Solin. c. LII. Polyb. Histor. p. 700.
[r] Joseph. Oper. Ox. 1720.

Dionyſius is ſuppoſed to have lived after Domitian, and before Severus [s]. He wrote a deſcription of the world in Greek verſe, which it may be ſuppoſed he had finiſhed before the reign of Trajan, or at leaſt that he had not heard of the increaſe of geographical knowledge which took place at that time, for he was as little acquainted with the country beyond the Ganges, as thoſe who are ſuppoſed to have been his predeceſſors, and only mentions it as an iſland remarkable for the diſtinctneſs with which the ſun-riſing was obſerved,

> Χρυσειην τοι νησον αγει πορος, ενθα και αυτου
> Αντολιη καθαροιο φαεινεται ηελιοιο.
>
> Dionyſ. Perieg. p. 111.

Ptolemy flouriſhed under Adrian, and Antoninus; and made his laſt aſtronomical obſervation on a Wedneſday, the 2d of February, in the year 141 [r]. He has taken notice of many places not mentioned any where elſe, and is the firſt who has called Malacca a peninſula. Marinus indeed, whom he quotes as a late author, knew likewiſe that it was ſo; which ſtill more confirms the ſuppoſition, that this was found out in Trajan's reign.

Ptolemy's works evidently ſhew, that his knowledge was ſuperior to that of all the other antient geographers; and his living in Ægypt gave him ma-

[r] Dodwell, in Geog. vet.
[s] Univerſal Hiſt. Vol. XV. p. 206.

ny oportunities of a very early intelligence concerning any discoveries made by navigation, which might be a long time before they were communicated to the other learned men of that extensive empire. Accordingly we see, that the author of the P. Maris Erythræi, who is supposed to have been his cotemporary [t], but lived a little later to the time of Marcus and Verus, was less acquainted with these late discoveries.

Agathemerus, who had read Ptolemy's works, lived in the reign of Severus and Galienus [u], in the begining of the third century, and mentions the country of the Sinæ as the most oriental he was acquainted with.

Marcianus Heracleota is the last geographical author it will be necessary to mention. He is supposed to have lived some little time before the building of Constantinople, and even at that time this nation appears to have been the most oriental; for, although he copied from such authors as wrote in the interval between Ptolemy and him, yet all the improvement that was made during that time was only a mensuration of this particular coast, which Ptolemy himself tells us was not done in the days in which he lived [x].

From these circumstances it is apparent, that no mention was made of this country during the first century. Marinus, as we have seen, wrote before Ptolemy; Ptolemy was far advanced in years before

[t] Dodwell.
[u] Dodwell.
[x] Ptol. c. 14.

the

the middle of the 2d century; and farther, as it may be ſuppoſed that Trajan ſent theſe ſhips to India at the time of his arrival in Arabia, which was in the 116th of the Chriſtian era; this may very well agree, in point of Chronology, not only with theſe authors, but alſo with our former ſuppoſition, that this country was found out in his reign. But as he ſcarce ſurvived the expedition two years, ſuch perſons as were employed in this voyage, finding on their return that he was dead, might be diſcouraged from purſuing any diſcoveries they had made: eſpecially as the voyage was attended with ſo much hazard and difficulty, and as the views upon which they had undertaken it were in all probability fruſtrated by the acceſſion of a new emperor.

Admitting therefore, that this was their firſt attempt, may not the extent of their diſcovery be looked upon as very conſiderable; and will it not in ſome meaſure account for their not having proceeded any farther than the eaſt ſide of the bay of Siam?

Upon the whole, as nothing was exported from this kingdom of the Sinæ but what the city of Cambodia excelled in; and as the ancient and modern ſituations of theſe cities appear to be reciprocal; above all, as we have the teſtimony of the Periplus Maris Erythræi, that it lay ſomewhere in the bay of Siam, and the expreſs declaration of two others, that it was ſituated on the eaſt ſide of the bay; joined to the unanimous conſent of all the geographers, that the country to the eaſt and ſouth was unknown, it may reaſonably be inferred, that their *ultima* were upon

 this

this coast; and the metropolis Sina or Thina the same as the modern city of Cambodia [y].

Greek-Street, Soho.
Feb. 1. 1767.

[y] Θινα by the author of the Periplus Maris Erythræi, p. 36. Σινων μετροπολις and Θειναι by Ptolemy, lib. i. c. 17. Θειναι by Marcianus Heracleota, p. 14. Θιν, acervus, cumulus, collis, &c. it might receive this name because it stands upon a mount, according to Mandesloe, vol. I. p. 331. to secure it from inundations. None of all these ancient writers of geography have ever called this country Θινων but Agathamerus, lib. ii. c. 7. and it would appear he did it by mistake; for its proper name was certainly Metropolis Σινων, and Θινα was only an accidental one, which it afterwards received to express more figuratively its situation.

Received February 12, 1767.

XVIII. *A Computation of the Distance of the Sun from the Earth. By* S. Horsley, *LL. B. Rector of* Saint Mary, Newington, *in* Surry, *F. R. S.*

Read March 26, 1767.

I Offer the following computation, rather as a verification than an amendment of Dr. Stewart's. The method, in which I have pursued, is different from what is used by that great and able geometrician, in his treatise on the distance of the sun, but founded entirely on the theorems established in that and the preceding tracts of the same author.

Let TA be a given line. Take A*m*, so that TA may be to A*m*, as the moon's accelerating attraction to the earth, to the sun's mean disturbance of that attraction. Take AG quintuple of A*m*. Take AP, such that twice A*m* may be to AP, as TG to TA. Now it is proved in the twenty-fifth proposition of Dr. Stewart's fourth tract, that the cube of TA is to the cube of T P, in the duplicate proportion of the periodic month to the anomalistic month. Therefore the proportion of TA^3 to TP^3, and consequently that of TA to TP, is given; and by division, that of TA to A P is given. Therefore TA being given, A P is given. Now $TG : TA = 2Am : AP$. That is,

 TA

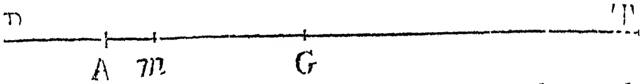
$TA - 5Am : TA = 2Am : AP$. Therefore $TA \times 2Am = \overline{TA - 5Am} \times AP$. Therefore $2Am = \frac{\overline{TA \times AP - 5Am \times AP}}{TA}$.

That is $2Am + \frac{5Am \times AP}{TA}$, or $\frac{\overline{2TA + 5AP} \times Am}{TA} = AP$.

That is,

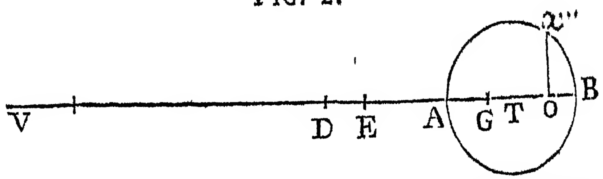
FIG. I.

$Am = \frac{TA \times AP}{2TA + 5AP}$.



2. Let TA represent the moon's mean distance from the earth. Take TV, such that TA may be to TV, in the duplicate proportion of the periodic month to the sidereal year. Take TG, such that VT may

FIG. 2.



be to TG in the proportion of the moon's accelerating attraction to the earth, to the sun's mean disturbance of that attraction. Take TE, such that TE may be to TA, as TA to TG. Take EO, such that the rectangle EOA may be equal to $3TE \times TA$. Upon the centre T, with the interval TA, describe a circle. Draw Ox perpendicular to AB, meeting the circle in x. Take $AD = AT$. The proportion of TA to TV being given, and TA being given, TV is given. But the proportion of TV to TG is given. Therefore TG is given, and the proportion of TG to TA is given. $TG : TA = TA : TE$. Therefore the proportion of TA to TE is given. Therefore TE is given. Therefore $3TE \times TA$ is given. Therefore $EO \times OA$ is given. And EA ($= TE - TA$) is given. Therefore AO is given. But AB ($= 2AT$) is given. Therefore OB is given. Therefore $AO \times OB$ is given. $AO \times OB = Ox^2$ (by the circle). Therefore Ox^2, and consequently Ox is given. But DB ($= 3AT$) is given. Therefore the proportion of DB to Ox is given.
And

And the proportion of DB to Ox, is that of the mean diſtance of the ſun, to the mean diſtance of the moon.

This is in brief the method of my computation. The computation is as follows:

The periodic month is to the anomaliſtic month, as 57600 to 58091.

Therefore (in Fig. 1.) $TA^3 : TP^3 = 57600^2 : 58091^2 = 3317760000 : 3374564281$

$$\frac{3374564281}{3317760000} = 1{,}01712127489\dot{6}3155\dot{8}64197530\dot{8}64197530, \text{ \&c.}$$

Hence, by extracting the cube root, I find $TA : TP = 1 : 1{,}0056748270 53$.

Therefore put $TA = 1$. Then $TP = 1{,}005674827053$; and $AP = 0{,}005674827053$.

$$\text{Hence } \frac{TA \times AP}{2TA + 5AP} = 0{,}002797722 = Am.$$

(See Fig. 2). The ſquare of the periodic month is to the ſquare of the ſidereal year, as 1 to 178,725.

Therefore $TA : TV = 1 : 178{,}725$.

But $TV : TG = 1 : 0{,}002797722$.

Therefore $TA : TG = 1 : 178{,}725 \times 0{,}002797722 = 1 : 0{,}50002286445$.

$TA : TG = TE : TA$. Therefore $TE : TA = 1 : 0{,}50002286445$.

Therefore put $TE = 1$.

Then $TA = 0{,}50002286445$

And $EA = 0{,}49997713555$

And $3TE \times TA = 1{,}50006859335 = EOA$.

$$\text{Hence } AO \left(= \sqrt{TE \times TA + \frac{EA^2}{4}} - \frac{EA}{2} \right) = 1{,}00003658292.$$

But $AB = 2TA = 1{,}00004572890$

Therefore $OB = 0{,}00000914598$

Therefore $AO \times OB = 0{,}000009146314586654 6616$

Therefore $\sqrt{AO \times O} = 0{,}co{,}32024287 = Ox$

But $DB = 3TA = 1{,}500068593$

Hence $DB : Ox = 496{,}0073 : 1$.

These

These computations have been made with no ſmall rigor. I was ſenſible that, to obtain an accurate concluſion, it was neceſſary to determine AO with extreme preciſion; and for that purpoſe I ſubmitted to the laborious taſk of computing the foregoing numbers to the 11th or 12th decimal place, by the common operations of arithmetic. In the reſult I differ from Dr. Stewart, by much leſs than $\frac{1}{6000}$th part of the whole diſtance, that is, by leſs than 5 ſemidiameters of the earth; a very contemptible difference in ſo nice a calculation. That great mathematician indeed ſeems to have flattered himſelf, that he had determined the ſun's diſtance within $\frac{1}{430000}$ of the truth: I ſuſpect that when he affirmed this, he did not conſider that to attain ſo great an accuracy in the concluſion, the line Eq in his method (vide Stewart on the ſun's diſtance, Fig. 10.), or AO in mine, ſhould be determined ſtrictly to the 11th or 12th decimal place. And after the utmoſt rigor of computation, I am afraid any pretenſions to ſuch extreme nicety in the reſult will be but ill-founded. For it is very likely that theſe computations repreſent the ſun's diſtance leſs than it really is: becauſe the whole progreſſion of the moon's apogee (which is the baſis of the calculation) is aſcribed to the ſun's diſturbance of the moon's gravitation to the earth. Whereas part of it muſt be due to the diſturbances of the planets. What part is due to them we cannot tell, and therefore cannot allow for it. But in giving the whole to the ſun we certainly overrate his diſturbing force, and by that means muſt obtain too ſmall a diſtance.

It

It is moſt likely indeed, that the motion of the apogee produced by the diſturbing forces of the planets bears but a very ſmall, perhaps inſenſible, proportion to the whole. But thoſe who are maſters of Dr. Stewart's theorems will eaſily perceive that an inſenſible error in the proportion of the moon's gravity, and the ſun's diſturbance, may produce a very ſenſible error in the proportion of the mean diſtances. And therefore the real diſtance is probably greater by two or three ſemidiameters of the earth than theſe computations make it.

This, however, is much too nice a point for the approaching tranſit, or, perhaps, for any method of obſervation, to determine. The higheſt expectations aſtronomers will be anſwered, if they can come within 50 or 60 ſemidiameters of the earth.

It is to be hoped, that every civilized nation of the univerſe will give due attention to that intereſting phænomenon, which we, the preſent poſſeſſors of theſe ſublunary regions, ſhall behold no more; and that proper perſons will be ſent in due time, and duly equipped, to the moſt advantageous ſtations.

If the deciſions of obſervation in ſo nice a point ſhould be found to agree with the previous concluſions of theory, the diſciples of Newton will have no ſmall reaſon to exult in a new atteſtation of nature, to the truth of their great maſter's doctrine.

But it is much to be wiſhed, that they, who ſhall be deputed to proſecute this curious ſearch, in diſtant and ſequeſtered parts, may diveſt themſelves of all prejudice; that they may have nothing at heart, but, that which the world will expect from them, the

the advancement of real ſcience; that they may be diligent in their obſervations, and faithful in their reports; and not ſacrifice the repoſe of their own minds, or the intereſts of philoſophy, to the credit of an admired hypotheſis, the memory of a friend, or the jealouſies of rival nations.

If the moon's mean diſtance from the earth be $60\frac{1}{3}$ ſemidiameters of the earth, the ſun's mean diſtance is 30008,4416 ſemidiameters of the earth.

The ſun's ſemidiameter is to the ſemidiameter of the earth, as 139,876 to 1. The globe of the ſun is to the globe of the earth, as 2736718,8 to 1; and the ſun's horizontal parallax is 6″ 52‴,415.

February 8.

To ſatisfy myſelf more fully of the accuracy of my work, I this day re-computed the whole, from the determination of EA, in Dr. Stewart's approximating method. I found the proportion of DB to *vs* (ſee Dr. Stewart on the diſtance of the ſun, Fig. 10.), that of 496,00579 to 1; and the proportion of DB to *tv*, that of 496,00805 to 1. The mean of theſe two gives the proportion of DB to *ox*, nearly that of 496,0069 to 1. Which differs from the reſult of my former computation by leſs than $\frac{1}{1240000}$ of the whole; and the method of the former computation is undoubtedly the moſt accurate.

Supplement

Supplement to the foregoing Paper.

Read June 19, 1767.

IN deducing the diſtance of the ſun in ſemidiameters of the earth, and his horizontal parallax, from the proportion above concluded between the ſun's mean diſtance and that of the moon; I have ſuppoſed the latter to be $60\frac{1}{2}$ ſemidiameters of the earth, as it is reckoned by Sir Iſaac Newton. According to the hypotheſis which ſeems to be now generally received, that the denſity of the moon is very nearly equal to that of the earth, (the French reckon it rather leſs), the moon's mean diſtance ſhould be little more than 60,23207, that is, not quite $60\frac{1}{4}$ ſemidiameters of the earth. But from ſome computations that I have formed with great care; I have reaſon to think, that Sir Iſaac Newton's determination is much nearer to the truth; that the denſity of the moon is actually greater than that of the earth, in the proportion of 6 to 5 nearly; and that the moon's mean diſtance amounts to 60,441 ſemidiameters of the earth; which differs from the diſtance aſſigned by Sir Iſaac Newton, by leſs than $\frac{1}{1007}$ of the whole.

S. Horſley.

Received February 12, 1767.

XIX. *Description of an improved Apparatus for performing Electrical Experiments, in which the Electrical Power is increased, the Operator intirely secured from receiving any any accidental Shocks, and the whole rendered more convenient for Experiments than heretofore: By* C. L'Epinasse, *F.R.S.*

Read March 12, 1767.

I. THE first method of improvement consists in lining the inside of the glass cylinder or globe with the following composition.

Take 4 ℔ of Venice turpentine, 1 ℔ of resin, 1 ℔ of bee's wax; boil these over a gentle fire, stirring them now and then, for about four hours, at the end of which, stirr in a quarter of a pound of vermilion: then, a little of the mixture being taken out and left to cool, will be hard and brittle; a token that it is fit for use. Having well heated your globe or cylinder, pour the melted mixture into it; turn the cylinder about so as to spread it evenly over the inside surface to the thickness of a sixpence, and let it cool very gradually.

The advantages that result from this are as follow.

1st, Upon repeated trials I have constantly found, that a cylinder thus lined acted with much greater force

force than it did before it was lined, every other circumſtance alike. When firſt I made this obſervation, it induced me to try what effect the lining would have upon ſome cylinders, which I had found ſo bad that I had laid them aſide as uſeleſs.

Upon being lined, they proved much better than any I ever had of the ſame ſize before.

2dly, Electrical machines, when laid by for any conſiderable time, are very apt to be out of order, and ſometimes require much trouble before they can be brought to act: this inconvenience is in a great meaſure removed by thus lining the glaſs.

3dly, The cylinders thus lined are by far leſs liable to break by any alteration of weather, or in working the machine, which often was the caſe with mine before I lined them.

4thly, As a ſmall cylinder thus prepared is equal in power to one much larger, that is not, and requires leſs friction, the apparatus in which it is mounted may be much contracted, and the whole, together with the perſon that turns the machine, may be eaſily ſupported upon one or two ſmall ſtools with glaſs feet, when experiments require it. With a lined cylinder $7\frac{1}{2}$ inches in diameter, and about 9 inches long, I have loaded three jars, that held four gallons each, to that degree as to burſt one of them, which made an exploſion near to that of a pocket piſtol. The cylinder was mounted in a braſs frame with a wheel and pinion; the wheel was turned with eaſe by a ſmall braſs winch, and the rotation of the winch to that of the cylinder was as one to three.

II. As raiſing the greateſt quantity of electrical fire was the object of the firſt improvement, the next

 thing

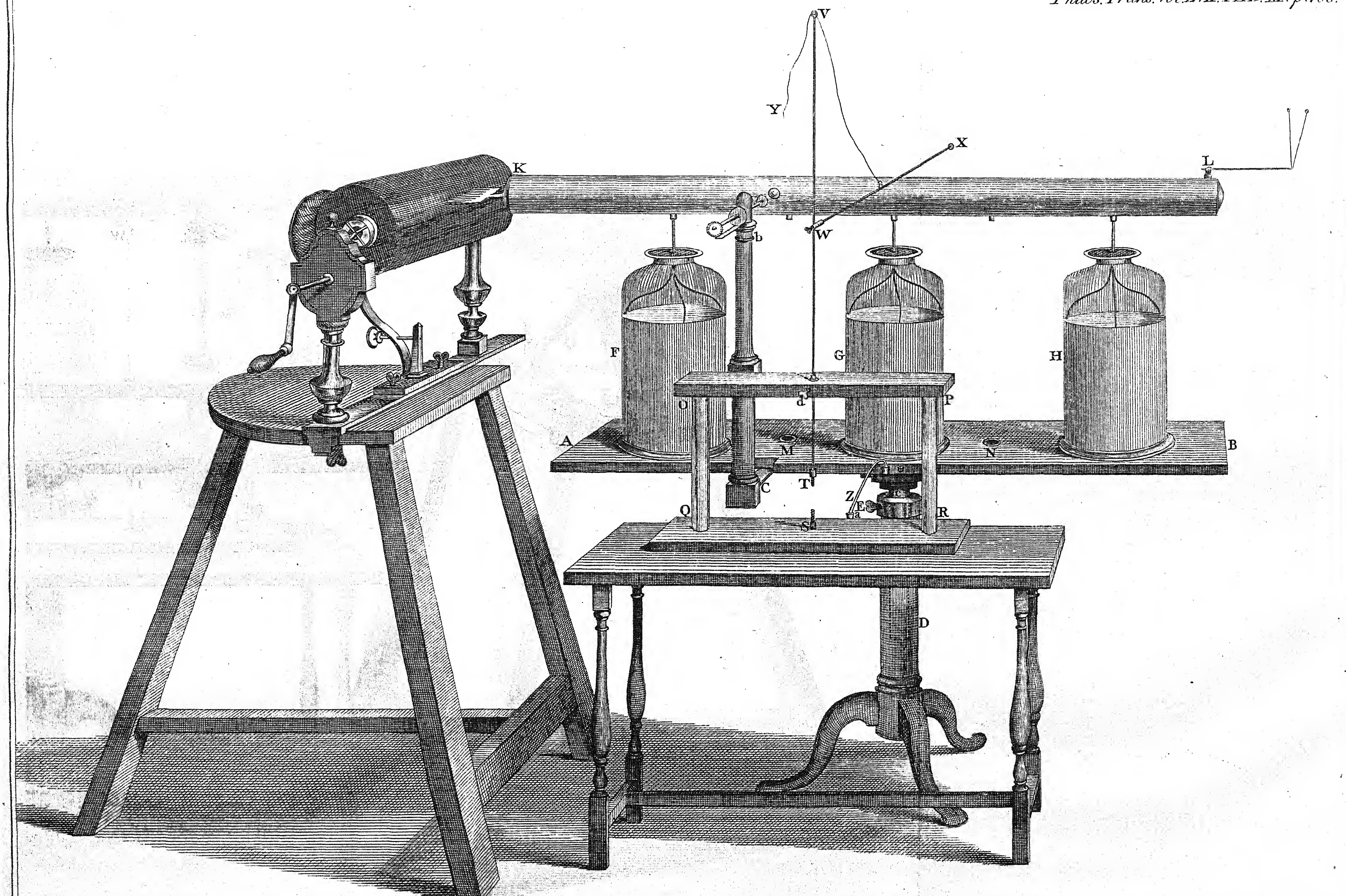
thing was, to preſerve it when raiſed, and uſe it without waſting any, ſo that it might have its full effect. I had obſerved, that whenever a ſingle wire was made uſe of inſtead of a chain in diſcharging the jars, the effect was much ſtronger; and upon making further experiments, I found that when the diſcharging parts were not all in cloſe contact, ſuch as being ſcrewed tight together, or ground into one another, the effect was conſiderably diminiſhed. In conſtructing the diſcharging apparatus, I therefore contrived that all the parts ſhould be in cloſe contact, by ſcrewing grinding, or ſoldering, them together; and thus the electrical fire exerts its whole force on the body upon which the experiment is made.

III. Laſtly, it often happened in diſcharging the jars when loaded very high (as they muſt be to kill a large animal, or to force the fire through bodies that make a great reſiſtance), that the perſons operating, notwithſtanding all their ſkill and care, received the whole or part of the ſhock. This has deterred many from repeating ſeveral uſeful experiments, and has intimidated others that attempted to repeat them ſo as to make them fail of their effect. To remove this inconvenience intirely, the diſcharging frame is contrived, which, at the ſame time that it prevents the waſting of the electrical fire, leaves no poſſibility of the operator's ever receiving any ſhock. This will plainly appear upon inſpecting the figure annexed.

EXPLANATION *of* TAB. XI.

AB, a mahogany board $4\frac{1}{2}$ feet long, which ſupports the jars F, G, H, and the conductor KL.

This



S. Wale delin. *J. Mynde sculp.*

This board has three large holes cut through it, to fit the three jars: over theſe holes, on the under ſide of the board, are nailed three tin plates, which communicate to one another by a ſlip of tin, and upon which the bottom of the jars reſt. Under the middle of this board is alſo fixed by a wooden ſcrew a cylindrical piece of wood *c*, which moves up and down in the hollow of the clawed pillar D, and may be ſtopt at any height by means of the ſcrew E; ſo that the whole apparatus can be raiſed to a height ſuitable to any electrical machine to which it is applied.

F, G, H, three glaſs jars, about ten inches diameter, and fourteen inches high, lined inſide and out with tin foyl to about two thirds of their height. A piece of wood is cemented at their top, through which there paſſes a thick braſs wire, one end of which fits into a ſocket ſoldered to the conductor, and to the other end within the jar are fixed ſmall wires, which ſpread and form a communication between the inſide lining of the jars and the conductor.

KL, the conductor, a tin tube about five feet long and three inches in diameter, cloſed and rounded off at each end. At the end next the machine is fixt a piece of braſs made of pointed wires in the ſhape of a comb, which collects the electrical fire from the machine; at the other end is fixt a ſlight piece of mahogany, at the end of which two ſmall cork balls are ſuſpended by fine threads, which balls by receding from each other, ſhew to what degree the power is raiſed; and when the jars are fully condenſed will ſtand nearly upright as in the figure.

When experiments require to draw a ſpark from the conductor without a ſhock, which we may call ſingle

fingle fparks, the jars are then removed, and the conductor is fupported by two glafs pillars, fuch as are ufed in the difcharging apparatus; one end of which fits into holes made in the board at M and N, and the other end having a wire fixt to it, fupports the conductor.

OPQR, the difcharging frame placed on a table, and confifting of the following pieces.

QSR, a mahogany board twenty inches long and feven broad: about the middle of this board a narrow brafs plate is let into the thicknefs of the wood, reaching from S to *a*. At the extremity *a*, which projects out of the board, there is a loop into which the end of the bent wire Z is fitted by being ground into it, the other extremity receives the end of the nut S.

S, a brafs nut made with a double fcrew, the lower end being a male fcrew, to faften it to the brafs plate; and the upper end a female fcrew, to receive the ends of different wires and other pieces which fit into it for performing various experiments.

OQ, PR, two glafs pillars about twelve inches long and an inch thick, made with fhoulders at the end, which fit into the board QR, and the rail OP.

OP, a mahogany rail three inches broad and $1\frac{1}{2}$ thick, nearly the fame length with the board QR, which receives the upper ends of the glafs pillars.

TV, a thick brafs wire, which paffes through the middle of the rail, and, by means of a fcrew at *d*, may be ftopt at any height. At the upper end V of this wire is a loop, and at the lower end T a fcrew, to which pieces are fixt to anfwer thofe in the nut S.

W, a folid piece of brafs, through which the upright wire TV paffes, which may be fhifted to any

part

part of this wire ſuitable to the height of the conductor, and fixt there by a ſcrew. A croſs wire WX is fixt to this piece with a moveable joint towards W, ſo that it may be drawn up or let down by the ſilk ſtring Y; one end of which is fixt to the croſs wire, and the other end paſſes through the loop V.

X, a ſmall braſs knob at the end of the wire.

Z, a bent wire, which forms the communication between the diſcharging frame and the outſide coating of the jars; one end of this wire is ground into a braſs loop at *a*, and the other into a loop of the ſame kind at *e*, which is ſoldered to the tin plate that lies under the jars.

b C, an electrometer, which may be fixt on occaſionally. When the diſcharging frame is uſed, the body upon which the experiment is made is placed between the nut S, and the end T of the upright wire, which is brought into contact with it; then the wire WX is drawn up to an erect poſition; and, after the jars are ſufficiently loaded, it is let down upon the conductor by the ſilk ſtring Y, and diſcharging the jars, the electrical ſpark in forming the circle paſſes through the body. Care muſt be taken to keep the glaſs pillars dry and clean, that none of the fire be waſted.

When an animal is to be killed, he muſt be faſtened to a board, and his head placed between the two points S, and T. If the diſcharging frame be removed, the apparatus may be uſed in medical caſes as uſual; either with an electrometer or without.

 XX. *Two*

XX. *Two Letters from the Hon.* William Hamilton, *His Majesty's Envoy Extraordinary at* Naples, *to the Earl of* Morton, *President of the Royal Society, containing an Account of the last Eruption of* Mount Vesuvius.

Naples, June 10, 1766.

My Lord,

Read March 5, and April 2, 1767.

AS I have attended particularly to the various changes of Mount Vesuvius, from the 17th of November 1764, the day of my arrival at this capital, I flatter myself, that my observations will not be unacceptable to your lordship, especially as this Volcano has lately made a very considerable eruption. I shall confine myself merely to the many extraordinary appearances that have come under my own inspection, and leave their explanation to the more learned in natural philosophy.

During the first twelvemonth of my being here, I did not perceive any remarkable alteration in the mountain; but I observed the smoke from the mouth of the Volcano was much more considerable in bad weather than when it was fair; and I often heard (even at Naples, six miles from Vesuvius) in bad weather, the report of the inward explosions of the mountain. When I have been at the top of Mount Vesuvius in fair weather, I have sometimes found so

little

little ſmoke that I have been able to ſee far down the mouth of the volcano, the ſides of which were incruſted with ſalts and minerals of various colors, white, green, deep and pale yellow. The ſmoke that iſſued from the mouth of the volcano in bad weather was white, very moiſt, and not near ſo offenſive as the ſulphureous ſteams from various cracks on the ſides of the mountain.

Towards the month of ſeptember laſt, I perceived the ſmoke to be more conſiderable, and to continue even in fair weather; and in october I perceived ſometimes a puff of black ſmoke ſhoot up a conſiderable height in the midſt of the white, which ſymptom of an approaching eruption grew more frequent daily; and ſoon after, theſe puffs of ſmoke appeared in the night tinged like clouds with the ſetting ſun.

About the beginning of November, I went up the mountain; it was then covered with ſnow, and I perceived a little hillock of ſulphur had been thrown up ſince my laſt viſit there, within about forty yards of the mouth of the volcano; it was near ſix feet high, and a light blue flame iſſued conſtantly from its top. As I was examining this phænomenon, I heard a violent report, and ſaw a column of black ſmoke followed by a reddiſh flame, ſhoot up with violence from the mouth of the volcano, and preſently fell a ſhower of ſtones, one of which falling near me, made me retire with ſome precipitation, and alſo rendered me more cautious of approaching too near, in my ſubſequent journeys to Veſuvius.

From november to the 28th of march, the date of the beginning of this erruption, the ſmoke encreaſed and was mixed with aſhes, which fell, and did great

 damage

damage to the vineyards in the neighbourhood of the mountain. A few days before the eruption I ſaw (what Pliny the younger mentions having ſeen, before that eruption of Veſuvius which proved fatal to his uncle) the black ſmoke take the form of a pine-tree. The ſmoke that appeared black in the day-time for near two months, before the eruption had the appearance of flame in the night.

On good friday, the 28th of march, at 7 o'clock at night, the lava began to boil over the mouth of the volcano, at firſt in one ſtream; and ſoon after, dividing itſelf into two, it took its courſe towards Portici. It was preceded by a violent exploſion, which cauſed a partial earthquake in the neighbourhood of the mountain, and a ſhower of red hot ſtones and cinders were thrown up to a conſiderable height. Immediately upon ſight of the lava, I left Naples with a party of my countrymen, whom I found as impatient as myſelf to ſatisfy their curioſity in examining ſo curious an operation of nature. I paſſed the whole night upon the mountain; and obſerved that, though the red hot ſtones were thrown up in much greater number and to a more conſiderable height than before the appearance of the lava, yet the report was much leſs conſiderable than ſome days before the eruption. The lava ran near a mile in an hour's time, when the two branches joined in a hollow on the ſide of the mountain, without proceeding farther. I approached the mouth of the volcano, as near as I could with prudence; the lava had the appearance of a river of red hot and liquid metal, ſuch as we ſee in the glaſs houſes, on which were large floating cinders half lighted, and rolling one over another

with great precipitation down the ſide of the mountain, forming a moſt beautiful and uncommon caſcade; the color of the fire was much paler and more bright the firſt night than the ſubſequent nights, when it became of a deep red, probably owing to its having been more impregnated with ſulphur at firſt than afterwards. In the day-time, unleſs you are quite cloſe, the lava has no appearance of fire; but a thick white ſmoke marks its courſe.

The 29th the mountain was very quiet, and the lava did not continue. The 30th it began to flow again in the ſame direction, whilſt the mouth of the volcano threw up every minute a girandole of red hot ſtones, to an immenſe height. The 31ſt I paſſed the night upon the mountain; the lava was not ſo conſiderable as the firſt night, but the red hot ſtones were perfectly tranſparent, ſome of which I dare ſay of a ton weight, mounted at leaſt 200 feet perpendicular, and fell in, or near, the mouth of a little mountain, that was now formed by the quantity of aſhes and ſtones, within the great mouth of the volcano, and which made the approach much ſafer than it had been ſome days before, when the mouth was near half a mile in circumference, and the ſtones took every direction. Mr. Hervey, brother to the earl of Briſtol, was very much wounded in the arm ſome days before the eruption, having approached too near; and two Engliſh gentlemen with him were alſo hurt. It is impoſſible to deſcribe the beautiful appearance of theſe girandoles of red hot ſtones, far ſurpaſſing the moſt aſtoniſhing artificial firework.

From the 31ſt of march to the 9th of april, the lava continued on the ſame ſide of the mountain in

two, three, and ſometimes four branches, without deſcending much lower than the firſt night. I remarked a kind of intermiſſion in the fever of the mountain, which ſeemed to return with violence every other night. On the 10th of april at night the lava diſappeared on the ſide of the mountain towards Naples, and broke out with much more violence on the ſide next the *Torre dell' Annunciata*.

I paſſed the whole day and the night of the 12th upon the mountain, and followed the courſe of the lava to its very ſource; it burſt out of the ſide of the mountain, within about half a mile of the mouth of the volcano, like a torrent, attended with violent exploſions, which threw up inflamed matter to a conſiderable height, the adjacent ground quivering like the timbers of a water-mill; the heat of the lava was ſo great as not to ſuffer me to approach nearer than within ten feet of the ſtream, and of ſuch a conſiſtency (though it appeared liquid as water) as almoſt to reſiſt the impreſſion of a long ſtick, with which I made the experiment; and large ſtones thrown on it with all my force did not ſink, but, making a ſlight impreſſion, floated on the ſurface, and were carried out of ſight in a ſhort time; for, notwithſtanding the conſiſtency of the lava, it ran with amazing velocity; I am ſure, the firſt mile with a rapidity equal to that of the river Severn, at the paſſage near Briſtol. The ſtream at its ſource was about ten feet wide, but ſoon extended itſelf, and divided into three branches, ſo that theſe rivers of fire communicating their heat to the cinders of former lavas, between one branch and the other, had the appearance at night of a continued ſheet of fire, four miles in length, and in ſome parts near

two

two in breadth. Your lordſhip may imagine the glorious appearance of this uncommon ſcene, ſuch as paſſes all deſcription.

The lava, after having run pure for about 100 yards, began to collect cinders, ſtones, &c. and a ſcum was formed on its ſurface, which in the day-time had the appearance of the river Thames, as I have ſeen it after a hard froſt and great fall of ſnow, when beginning to thaw, carrying down vaſt maſſes of ſnow and ice. In two places the liquid lava totally diſappeared, and ran in a ſubterraneous paſſage for ſome paces, then came out again pure, having left the ſcum behind. In this manner it advanced to the cultivated parts of the mountain; and I ſaw it the ſame night of the 12th, unmercifully deſtroy a poor man's vineyard and ſurround his cottage, notwithſtanding the oppoſition of many images of St. Januarius, that were placed upon the cottage, and tied to almoſt every vine. The lava, at the fartheſt extremity from its ſource, did not appear liquid, but like a heap of red hot coals forming a wall, in ſome places ten or twelve feet high, which rolling from the top ſoon formed another wall, and ſo on, advancing ſlowly not more than about thirty feet in an hour.

The mouth of the volcano has not thrown up any large ſtones ſince the ſecond eruption of lava, on the 10th of april, but has thrown up quantities of ſmall aſhes and pumice ſtones, that have greatly damaged the neighbouring vineyards. I have been ſeveral times at the mountain ſince the 12th; but as the eruption was in its greateſt vigour at that time, I have ventured to dwell on, and I fear tire your lordſhip with, the obſervations of that day.

In my laſt viſit to Mount Veſuvius the 3d of June, I ſtill found that the lava continued, but the rivers were become rivulets and had loſt much of their rapidity. The quantity of matter thrown out by this eruption, is greater than that of the laſt in the year 1760, but the damage to the cultivated lands is not ſo conſiderable, owing to its having ſpread itſelf much more, and its ſource being at leaſt three miles higher up. This eruption ſeems now to have exhauſted itſelf; and I expect in a few days to ſee Veſuvius reſtored to its former tranquillity.

Mount Etna in Sicily broke out the 27th of april, and made a lava in two branches, at leaſt ſix miles in length, and a mile in breadth, and, according to the deſcription given me by Mr. Wilbraham, who was there, after having ſeen with me part of the eruption of mount Veſuvius, reſembles it in every reſpect, except that mount Etna, at the place from whence the lava flowed (which was twelve miles from the mouth of the vulcano), threw up a fountain of liquid inflamed matter to a conſiderable height; which, I am told, mount Veſuvius has done in former eruptions.

I beg pardon for having taken up ſo much of your time, and yet I flatter myſelf, that my deſcription, which I aſſure your lordſhip is not exaggerated, will have afforded you ſome amuſement. I have the honour to be,

My Lord,

Your Lordſhip's moſt obedient

and moſt humble ſervant,

William Hamilton.

SINCE

Naples, February 3, 1767.

Read April 2, 1767.

SINCE the account of the eruption of mount Vesuvius, which I had the honour of giving to your lordship, in my letter of the 10th of june last, I have only to add, that the lava continued till about the end of november, without doing any great damage; having taken its course over antient lavas. Since the cessation of this eruption, I have examined the crater, and the crack on the side of the mountain towards *Torre dell' Anonciata*; about a hundred yards from the crater from whence this lava issued: and I found therein some very curious salts, and sulphurs; a specimen of each sort I have put into bottles myself, even upon the mountain, that they might not lose any of their force, and have sent them in a box directed to your lordship, as you will see by the inclosed bill of lading. I am sure you will have a pleasure in seeing them analyzed. I have also packed in the same box some lava, and cinders, of this eruption; there is one piece in particular very curious, having the exact appearance of a cable petrified. I shall be very happy if these trifles should afford your lordship a moment's amusement.

It is very extraordinary, that I cannot find, that any chemist here has ever been at the trouble of analyzing the productions of Vesuvius.

The deep yellow, or orange-color salts, of which there are two bottles, I fetched out of the very crater of the mountain, in a crevice that was indeed very hot. It seems to me to be powerful, as it turns silver black

black in an inſtant, but has no effect upon gold. If your lordſhip pleaſes, I will ſend you by another opportunity ſpecimens of the ſulphurs and ſalts of the *Solfa terra*, which ſeem to be very different from theſe.

Within theſe three days, the fire has appeared again on the top of Veſuvius, and earthquakes have been felt in the neighbourhood of the mountain. I was there on ſaturday with my nephew lord Greville; we heard moſt dreadful inward grumblings, rattling of ſtones, and hiſſing; and were obliged to leave the crater very ſoon, on account of the emiſſion of ſtones. The black ſmoke aroſe as before the laſt eruption; and I ſaw every ſymptom of a new eruption, of which I ſhall not fail to give your lordſhip an exact account.

XXI.

Received January 30, 1767.

XXI. *Extract of a Letter from* John Howard, *Esq; F. R. S. to* William Watson, *M. D. F. R. S. giving some Observations on the Heat of the Waters at* Bath.

Read April 2, 1767.

WHEN I had the pleasure of seeing you in London, you thought some explanation of the paper, upon the heat of the Bath-waters, necessary. The observations, I assure you, were made with great care. The three first of them were made at the pump, where the waters are usually drunk. I went several times into the king's, and queen's, baths; and took them, where the springs rise in the king's bath, which is the warmest part, and the most distant from thence the coolest. The pump in that bath corresponded with the upper pump. This I mention; as in the other two baths, the thermometers did not rise, by one degree, so high as in the upper pumps; though I had the pumps worked a considerable time, to warm the pipes. The temperature of the other baths you see in my paper. The hot bath, not being so warm as the king's, at first, surprized me, until I considered their times of filling; and then the comparative slowness of the springs of the hot bath, to those of the king's, accounted for this difference.

The other observations are of various springs, in and about Bath; that near St. James's church is the coldest

coldeſt. The ſprings, at the old and new well houſes at Briſtol, are very different; but, on repeated trials, I found them ſo.

Obſervations on the Heat of the Bath Waters.

King's bath pump	113°			
Hot bath pump	114			
Croſs bath pump	108			
King's bath *	99	97	100	cooleſt part
	101	99	103	hotteſt part
Queen's bath *	97	95	98	cooleſt part
	98	96	99	warmeſt
The pump in the bath	113			
Croſs bath	89	cooleſt part		
	90	warmeſt part		
Croſs bath pump	107			
Hot bath	96	cooleſt part		
	97	warmeſt part		
The pump in the hot bath	113			
Pump in the Market-place, Bath	54			
Springs on Claverton, and at late Mr. Allen's	47			
Springs on Lanſdown	45			
St. James's ſpring water	43			
Old well houſe, Briſtol	67			
New well, ditto	76			

The temperature of the above ſprings taken in November, and December laſt 1765, by Farenheit's ſcale (Bird's Thermometer).

* Taken at three different days.

John Howard.

 Bath,

XXII. *Observations on the Heat of the* Bath *and* Bristol *Water, by Mr.* John Canton, *A. M. F. R. S.*

Bath, Sept. 2, O. S. or the 13th N. S. 1752.

Read July 3, 1767.

AFTER pumping about $\frac{1}{4}$ of an hour, a Fahrenheit's thermometer, held in the ſtream from the common pump of the king's bath, was raiſed to 112°. The ſtream from the common pump of the hot bath raiſed it to $114°\frac{1}{2}$. At the pump of the croſs bath, it ſtood at 110°. At noon, the heat of the ſhaded air was 66°, and of common water expoſed to it 61°. I found the bath water, and common water, brought to the ſame degree of heat, to cool equally faſt. The next day, Sept. 14, I was at the hot-well near Briſtol, the water of which raiſed the thermometer to 76°. In common water expoſed to the ſhaded air it ſtood at 62°.

J. Canton.

Received April 3, 1767.

XXIII. *A Letter to Dr.* William Watſon, *F. R. S. from the Hon.* Daines Barrington, *F. R. S. on ſome particular Fiſh found in* Wales.

Dear Sir,

Read April 9, 1767.

KNOWING your attention to every thing which relates to any branch of natural hiſtory, I ſhall not make apologies for ſending you an account of the following particulars with regard to perch in a pool of Merionethſhire; and trout, which are found in a river of Cardiganſhire.

The pool is ſituated in the pariſh of Trawſvynnyd, and is called Llyn Raithlyn; as it does not lie near any road, a common traveller cannot hear any thing about it, but by very extraordinary acccident.

Having been informed by Mr. Garnons of Riwgoch, who lives near this pool, that perch were frequently caught there, which were crooked near the tail, I have, through him, procured fiſh of this ſort at three different times; as I intended to preſerve them in ſpirits, I have always deſired that they ſhould be of a ſmall ſize.

Theſe fiſh were all of them moſt apparently crooked in that part; which appears ſtill ſtronger (as I am informed) in thoſe of a larger ſize, and ſome of them have been taken of nearly two pounds.

I have

I have never examined the back bone of theſe perch, but I have now by me that of one of the trout (which I mean likewiſe to give you an account of): this bone, any one may immediately ſee, differs moſt apparently from that of a common trout, or any other fiſh, by its being crooked near the tail. I have therefore no doubt but that the back bone of theſe perch will turn out to be equally crooked.

Theſe fiſh are not only crooked near the tail, and for about one third of the whole length of their body; there is likewiſe a very remarkable protuberance on each ſide, which I have opened with a knife, but did not obſerve it to differ materially from other parts of the fleſh. I have likewiſe eat theſe perch, and ſhould not, by the taſte, have diſtinguiſhed them from the common ones of the ſame kind.

I happened likewiſe (after theſe inquiries with regard to the perch of Llyn Raithlyn) to hear of trout, which were crooked in the ſame part, ſaid to be peculiar to the river Eynion in Cardiganſhire, which is a ſmall brook, that empties itſelf into the Dovey, near Egglwys Vach, and is on the road from Machentleth in Montgomeryſhire, to Talypont in Cardiganſhire *.

* In Dalekarlia, a province of Sweden, near Fahlun, are two ſmall lakes, famous for the ſingular ſhape of the perch, wherewith they abound. Theſe perch grow to the common ſize, and are of a good taſte, but they have all a hump on their back. This peculiarity is taken notice of in Linnæi Fauna Suecica,† p. 118. The country people in the neighbourhood imagine that it may be occaſioned by the quality of the water in thoſe lakes, which

† In ſtagnis Fahlunæ hujus piſcis (Percæ) varietas eſt, quæ ſpinâ recurvâ, & corpore omninò gibbo, frequens reperitur.

I have

I have procured at two ſeveral times ſpecimens of theſe trout likewiſe, and have one now by me in ſpirits. They are crooked in the ſame manner near the tail; but, as the make of a trout is more taper than that of a perch, the curve does not appear ſo ſtrongly: no one, however, who looks at them with any degree of attention, can have the leaſt doubt of their differing moſt materially, from other fiſh of the ſame kind.

Theſe trout are caught, only in a ſmall baſon of perhaps eight or nine feet deep, which the river Eynion forms after a fall from the rocks. I have been informed, that, in a calm day, you may often ſee them in this baſon; it hath ſo happened that a good deal of wind hath ruffled the ſurface, when I have examined it.

I have only to add to theſe particulars, that, by very accurate accounts from thoſe, who have caught both the perch and trout, it is not above half of theſe fiſh which are thus crooked; and that the others do not in any reſpect differ from the common ones of theſe two ſorts.

As I have often obſerved that the exiſtence of ſuch fiſh was doubted by the Welſh themſelves, till I had procured theſe ſpecimens, it hath occaſioned my

might probably be impregnated with ſome mineral ſalt, eſpecially as they are ſituated near the largeſt copper mine in Europe.

Dan. Solander.

There is no copper mine near Llyn Raithlyn, or the river Eynion.

Daines Barrington.

inquiring

inquiring with regard to monocular fiſh, which are ſaid by Giraldus Cambrenſis, to be found in the lakes of Snowden, and from whom I ſhall tranſcribe the whole paſſage:

" In ſummis autem montium iſtorum verticibus, " duo lacus reperiuntur, ſuâ non indigni admirati- " one: alter enim inſulam habet erraticam, ad oppo- " ſitas plerumque partes errabundam; alter vero miro " & inaudito miraculo pollet, quia cùm trium gene- " rum piſcibus abundet, anguillis, truttis, & perchiis, " omnes in eo piſces monoculi reperiuntur, oculum " dextrum habentes, & ſiniſtro carentes: ſi rei tam " novæ, tamque ſtupendæ rationem ſcrupuloſus lector " efflagitet, aſſignare non præſumo." Giraldus Cambrenſis, lib. ii. cap. 10.

This writer was Archdeacon of Brecknock, and attended Baldwin Archbiſhop of Canterbury, in a progreſs which he made in the year 1188 through South and North Wales, to recommend a collection for a cruſade which was then in agitation.

We have no account of any part of England nearly ſo ancient, Leland's *Itinerary* having been undertaken only in the reign of Henry the VIIIth: it is not however merely the antiquity of the work, which ſhould recommend Giraldus's obſervations to the peruſal of every Engliſh reader; they are at the ſame time very entertaining, nor is the latinity contemptable.

Giraldus hath in common with other ancient travellers been conſidered as taking the liberties, with which they have been ſo frequently charged. For my own

own part the laſt undoubted diſcovery of the Patagonian giants, mentioned, but not credited in any previous account, will teach me not to diſbelieve entirely what is not a contradiction, on the very ſtate of it. I therefore do not abſolutely diſbelieve Mr. Groſe's late deſcription of pigmies which are found in a foreſt of the Carnatic, though I admit ſuch facts require the ſtrongeſt teſtimony before one ſhould give a complete aſſent. In ſhort, I am neither for implicit belief or diſbelief of ſuch extraordinary facts; and it is remarkable that Ariſtole, in his account of a nation of pigmies, ſays, " this is not a fable, but a truth."

Εϛι δε ὁ τοπος οὗτος, περι ὃν οἱ Πυγμαιοι κατοικουσιν· ου γαρ ἐϛι τουτο μυθος, αλλα εϛι κατα την αληθειαν, γενος μικρον μεν (ὥσπερ λεγεται), και αυτοι, και οἱ ἱπποι. Ariſt. de Nat. anim. l. viii. cap. 12.

But to return to the extract from Giraldus which hath been looked upon as one of the moſt glaring falſities, in this traveller.

It will appear to any one who reads the whole of his *Iter*, and is at all acquainted with the geography of the country, that Giraldus (who was a native of Pembrokeſhire) never was himſelf in theſe mountains of Snowden; he had therefore only picked up this account, from ſome of the inhabitants of the towns, through which the Archbiſhop paſſed, who themſelves probably received it from mountaineers.

There are few inhabitants of the principality, who have ever been in this tract of mountains; and I, who have been in moſt parts of them, have always been informed, at my ſetting out, that the roads were nearly unpaſſable.

Upon

Upon theſe occaſions, I have frequently inquired whether there was any ſuch notion or tradition amongſt the mountaineers, with regard to monocular fiſh, and have found, that it is ſuppoſed there are ſuch in a pool called Llyn y Cwn, which indeed I have never ſeen; but, by the beſt accounts I can procure, it is high up the Glyder mountain, which forms the oppoſite ſide of the vale of Lanberris to Wyddva, or the higheſt part of Snowden.

I have, by means of an interpreter, examined ſome of theſe mountaineers very particularly with regard to this point. One of them told me that, though he had often heard of theſe monocular fiſh, yet he had ſeen two or three taken by an angler in Llyn y Cwn, which did not ſeem to differ from common trout; and indeed the fiſh of that lake are ſeldom caught, as they are of very difficult acceſs, and have no extraordinary character either for goodneſs or ſize.

Mr. Hughes (of Penrhyn in Carnarvonſhire) having known that I had a curioſity with regard to theſe extraordinary fiſh, ſent me over laſt ſummer a mountaineer, whoſe account of them I took down by means of an interpreter, and ſend you herewith.

" Thomas William, of the pariſh of Lanleched, " taylor, aged 51, remembers one Daniel Pritchard of " Comb Cloran, yeoman, who was near 100 years " of age, and died in 1764. This Daniel Pritchard " told him (about nineteen years ſince), that he " caught a trout at Llyn y Cwn with one eye only in " the forehead, and that the head was thicker than " the heads of trout commonly are. He added alſo, " that William Robert, a fuller, had heard the ſame " ſtory from Pritchard: Thomas William himſelf,

 " however

"however informed me, that he had been fiſhing in "this pool, but never caught a trout of this extraordinary kind. He had likewiſe fiſhed in Llyn Marchleyn, and Llyn Bochlwyd, and though he caught "ſome fiſh with crooked backs, never took any that "had but one eye. He concluded his account by "ſaying, that there is a brother-in-law of Pritchard "now alive, who will confirm Pritchard's teſtimony; "and that it is generally believed in Lanleched "pariſh, there are fiſh which have but one eye, in "ſome of the pools of the neighbouring mountains."

If by this I have not eſtabliſhed the exiſtence of ſuch monocular fiſh in the Carnarvonſhire lakes, I hope you will at leaſt think that Giraldus's account is not to be ſo immediately rejected as impoſſible, eſpecially when I ſend you an extract from the Hiſtory of the French Academy of Sciences, which contains the following proof of nearly as extraordinary fiſh in a lake of France.

"Monſieur le Marquis de Montalembert a fait à "l'Académie l'obſervation ſuivante. Dans la fontaine "du Gabard en Angoumois, on pêche ſouvent des "brochets aveugles, & jamais aucun qui ne ſoit "borgne. Ceux qui ne ſont que borgnes, le ſont tous "de l'oeil droit, & dans ceux qui ſont aveugles on voit "aiſément que l'oeil droit a été attaqué le premier, "& eſt beaucoup plus endommagé que l'autre. "Cette fontaine eſt une eſpèce de gouffre, dont on ne "peut trouver le fond; pluſieurs petites iſles de "roſeaux, qui flottent à ſa ſurface, empêchent qu'on "ne puiſſe ſe ſervir de filets, pour y pêcher, ce qui "rend cette pêche très longue, & très difficile. "M. de Montalembert fut aſſez heureux pour at-

"traper un jeune brochet, qui effectivemeut ſe trouva
"borgne du coté droit. Ce qu'il y a de ſingulier
"c'eſt que cette fontaine ſe décharge par un aſſez
"gros ruiſſeau dans la Liſſonne, & que malgré cette
"communication, qui eſt très facile, les gens du païs
"aſſurent qu'on ne prend jamais dans cette riviere de
"brochets borgnes, ou aveugles, & qu'on n'en prend
"aucuns dans la fontaine qui ne le ſoient."

I refer you for this extract to page 27 and 28, of the Hiſtory of the Académy of Sciences for the year 1748, being the quarto edition, which you was ſo good as to lend me on this occaſion *.

The latter part of this extract, which aſſerts that theſe blind pike are only to be found in the pool of Gabard, and not in the ſmall river by which it communicates with the Liſſonne, ſuggeſts to me that it is generally ſuppoſed (and even by Lhwyd in his additions to Cambden's Britannia) ſalmon are never caught in the lake of Bala in Merionethſhire, though they are frequently taken in the river Dee, juſt below where it iſſues from that lake; whilſt the contrary is obſerved with regard to the fiſh called a Gwyniad, which is at the ſame time conceived to be peculiar to this lake.

I happened myſelf once to ſee a ſalmon of about fifteen pound, caught in the lake, at leaſt 200 yards

* I am likewiſe referred by an ingenious friend to a paſſage in Fr. Ern. Bruckmanni Epiſtolâ Itinerariâ xxxvi. Wolfenb. 1734, p. 10, which mentions a river in Germany, having all the trout blind: "Truttæ omnes (teſte P. Stephan. Amiodt, de Germaniâ "in naturæ operibus admirandâ, p. 66.) in flumine *Fiſchau* prope "*Mandorf* viſu deſtitutæ dicuntur. Vide plura apud Kirkelbeck, "p. 809, E. Brown in Itin. p. 196. and Math. Puel in Iten. "Thalaſſico, p. 33."

 above

above the bridge through which the river Dee iſſues; and though I never ſaw the Gwyniad taken in the Dee, yet I was moſt authentically informed by a gentleman who was preſent, that ſeveral of them were caught within theſe three years as low down as Landrillo, which is near ſix meaſured miles from the lake of Bala.

With regard to the Gwyniad's being peculiar to this lake, which is ſo generally believed, I can myſelf moſt flatly contradict this notion, as the firſt parcel of fiſh, which I happened to ſee in the market at Perith, in Cumberland, were of this ſort, and were brought from the Ulles Water, which is a large lake not above four or five miles diſtant from that town.

I have been likewiſe informed, by ſome people, who lived on the banks of Loch Lomond in Scotland, that fiſh anſwering to my deſcription of a Gwyniad were often caught in that lake. I did not happen however, myſelf, to ſee any of this ſort.

There is indeed one very ſtriking mark in this fiſh, which cannot but be attended to by thoſe even who are not naturaliſts: they have ventral fins of a very deep blue; and the belly, at moſt ſeaſons, is marked with ſmall blue ſpecks, which I do not recollect to have obſerved in any other fiſh of this iſland.

I have thus endeavoured to contribute my poor mite with regard to the natural hiſtory of Wales, which I have been enabled to ſtate with ſome preciſion, from having had occaſion to go twice a year into the principality, and to ſtay ſix or ſeven days in a particular place.

When I have mentioned to ſome friends, who are verſed in botany, and the ſtudy of natural hiſtory, the neceſſity

neceſſity of a more accurate and complete hiſtory of this kind; I have been frequently anſwered, that they deſpaired of making any material additions to the diſcoveries of Ray and other naturaliſts, who have been in many parts of this country.

It is very true that Ray and ſome of his contemporaries were in many of the Welſh counties; it appears, however, by his Journal lately publiſhed, that he ſtayed but a very ſhort time at any place, and only went to the top of ſome of the high mountains.

Now, Sir, I need not ſay, to a perſon of your conſummate knowledge in botany, that to diſcover all the plants of a country, one ſhould at leaſt, once a fortnight, ſearch every part, from the time vegetation commences, to that period of the autumn when it ceaſes. He who ſearches for pilewort in july, without knowing that it is a plant, which appears early in the ſpring, and that both leaves and flowers immediately afterwards decay, will ſearch in vain: the botaniſt muſt look for it in april, or not at all.

Dillenius is the lateſt botaniſt of reputation, who hath viſited theſe mountains; as I remember to have ſeen him a year or two before his death, I can venture to ſay that thoſe who may afterwards make the ſame ſearch, need not abſolutely deſpair of new and material diſcoveries, notwithſtanding his known diligence and accuracy.

He was of a very improper make for clambering up a rock, or mountain; and indeed it is a misfortune to the naturaliſt, that when from experience his knowledge becomes conſiderable, his ſtrength and activity begin to fail.

When

When a ſtranger, from botanical or other curioſity, goes to the top of a Welſh mountain, he is obliged to truſt implicitly to his guide, who contrives to carry him where he can ride on horſeback. The ſtranger therefore proceeds in the very track, where a ſheep or cow can graze, which conſequently crop the flowering ſtem and leaves of the plants.

It is in the fiſſures of rocks, and the central parts of bogs, that the ſearch for uncommon plants muſt be made; he who looks elſewhere for them, may travel a great deal of ground, but will never make any material diſcoveries.

I will not detain you longer with obſervations of this kind, as they muſt have been to you long obvious and familiar. I have, however, been inſenſibly betrayed into this length, by thinking, that every attempt to render the natural Hiſtory of Great Britain more complete, and accurate, may not be entirely without its uſe.

I am,

Dear Sir,

Your moſt faithful

Humble ſervant,

Daines Barrington.

XXIV. *An*

XXIV. *An Observation of an Eclipse of the Sun at the Island of* New-found-land, *August* 5, 1766, *by Mr.* James Cook, *with the Longitude of the Place of Observation deduced from it: Communicated by* J. Bevis, *M. D. F. R. S.*

Read April 30, 1767.

MR. Cook, a good mathematician, and very expert in his business, having been appointed by the Lords Commissioners of the Admiralty, to survey the sea coasts of New-found-land, Labradore, &c. took with him a very good apparatus of instruments, and among them a brass telescopic quadrant made by Mr. John Bird.

Being, August 5, 1766, at one of the Burgeo Islands near Cape Ray, latitude 47° 36′ 19″, the south-west extremity of New-found-land, and having carefully rectified his quadrant, he waited for the eclipse of the sun; just a minute after the beginning of which, he observed the zenith distance of the sun's upper limb 31° 57′ 00″; and, allowing for refraction and his semidiameter, the true zenith distance of the sun's centre 32° 13′ 30″, from whence he concluded the eclipse to have begun at 0^h 4′ 48″ apparent time, and by a like process to have ended at 3^h 45′ 26″ apparent time.

N. B. There

N.B. There were three ſeveral obſervers, with good teleſcopes, who all agreed as to the moments of beginning and ending.

Mr. Cook having communicated his obſervation to me, I ſhewed it to Mr. George Witchell, who told me he had a very exact obſervation of the ſame eclipſe, taken at Oxford by the Rev. Mr. Hornſby; and he would compute, from the compariſon, the difference of longitude of the places of obſervation, making due allowance for the effect of parallax, and the earth's prolate ſpheroidal figure; and he has ſince given me the following reſult:

5h	23′	59″	beginn. at Oxford.	7h	7′	5″	end at Oxford.
0	46	48	beginn. at Borgeo Iſles.	3	39	14	end at Borgeo Iſles.
4	37	11		3	27	51	
—	51	59	effect of parallax, &c.	+	17	35	effect of parallax, &c.
3	45	22	diff. of meridians.	3	45	26	diff. of meridians.

J. Bevis.

XXV. *Letter*

XXV. *Letter to Mr.* Dacosta, *Librarian, &c. to the Royal Society, from Mr.* William Martin; *containing an Extract of a Letter from his Son at* Bengal, *on the Heat of the Climate.*

S I R,

Read April 30, 1767.

THE candid reception I met with, when I took the liberty of addressing you in November 1764 by my friend John Ellis, Esq; a case somewhat uncommon, emboldens me, at this time, to communicate part of a letter I have lately received from my son, Fleming Martin, Esq; chief engineer at Bengall, dated 1st october 1765; giving an account of the incredible heat attending that climate, with some other observations, &c. If you think it worthy of being communicated to the Royal Society, it will afford me the greatest pleasure; but this I must beg leave to submit to your judgment, and am, with great respect,

Sir, your most obliged

humble servant,

Shadwell, 30 April, 1766.

William Martin.

Copy of part of a Letter, from Fleming Martin, *Esq;* *chief Engineer at* Bengall, *dated 1st* October, 1765.

IN regard to the intenſe and uncommon heat in this climate; it has been for ſome time paſt almoſt inſufferable.

The thermometer was ſeldom under 98, and the quickſilver roſe at certain times of the day to 104 degrees, by the beſt adjuſted inſtrument; nay, I have been aſſured by ſome gentlemen, that, in the camp 500 miles diſtant, the thermometer often ſtood at 120; but ſuch a difference, I imagine, was occaſioned by the badneſs of the inſtrument.

However it is certain, that nothing could exceed the intenſe heat we felt day and night, during the month of june. May and july were little inferior at times, but afforded ſome intermiſſion; otherwiſe a very great mortality muſt have attended this ſettlement, though we were not without inſtances of fatal effects in the month of june, when ſome few individuals in ſound health were ſuddenly ſeized, and died in the ſpace of four hours after; but, conſidering the malignity of the climate, we have not loſt many, and I believe the generality of people are not ſo intemperate as ſome years paſt they uſed to be; though, from what I have ſeen, the beſt conſtitutions in the moſt moderate perſons are a poor match againſt a fever or other diſorders in this country.

I have been as free from ſickneſs, as any other perſon in the ſettlement; but I cannot ſay that I have enjoyed myſelf in that degree as to be an exception; for no man here is without complaints, and life and

death

death are ſo ſuddenly exchanged, that medicines have not time very frequently to operate before the latter prevails. This is generally the caſe in malignant fevers, which are here termed *pucker fevers*, meaning (in the natives language) ſtrong fevers.

The rains have ſet in ſince the 4th of june. We call this the unhealthy ſeaſon, on account of the ſalt petre impregnated in the earth, which is exhaled by the ſun, when the rain admits of intervals. Great ſickneſs is cauſed thereby, eſpecially when the rains ſubſide; which generally happens about the middle of october. The air becomes afterwards rather more temperate, and, till april, permits of exerciſe, to recover the human frame that is relaxed and worn out by the preceding ſeaſon; for in the hot periods every relief is denyed, except riſing in the morning, and being on horſe back by day break, in order to enjoy an hour, or little more, before the ſun is elevated: it becomes too powerfull by ſix o'clock to withſtand its influence; nor can the ſame be attempted that day again till the ſun retires, ſo that the reſt of the twenty-four hours is paſſed under the moſt ſevere trials of heat. In ſuch ſeaſon it is impoſſible to ſleep under the ſuffocating heat that renders reſpiration extremely difficult; hence people get out into the virando's and elſewhere for breath, where the dews prove cooling, but generally mortal to ſuch as venture to ſleep in that air. In ſhort, this climate ſoon exhauſts a perſon's health and ſtrength, though ever ſo firm in conſtitution, as is viſible in every countenance, after being here twelve months. I have been lately informed by an officer of diſtinction, who was formerly engineer at this place, that he being ſent out to ſurvey

 a ſalt

a ſalt lake in the month of ſeptember, he found the ſulphureous vapours ſo ſtagnated and groſs, that he was obliged to get up into the talleſt trees he could find, to enjoy the benefit of reſpiration every now and then; he added that he conſtantly had recourſe to ſmoaking tobacco, (except during the hours of ſleep), to which and to ſwallowing large quantities of raw brandy (though naturally averſe to ſtrong liquors), he attributed his ſafety. However, on his return, he was ſeized with an inveterate fever of the putrid kind, which he miraculouſly ſurvived, though others, who attended him on the ſurvey, and had lived many years in the climate, were carried off, at the ſame time, by the like fever.

Received

Received February 5th, 1767.

XXVI. *Experiments on the Peruvian Bark, by* Thomas Percival, *M. D. F. R. S.*

Experiment I.

Read March 7, 1767.

AN ounce of Bark coarfely powdered was divided into two equal parts, one of which was infufed forty-eight hours in cold fpring water; the other was boiled over a flow fire forty minutes, until about a third part of the water was evaporated. The infufion and decoction were each filtered through linnen rags doubled and of equal finenefs.

Four grains of *Sal Martis* were diffolved in an ounce of fpring water, and one dram of this folution was added to equal quantities, viz. half an ounce; of the turbid decoction and infufion. Each affumed a deep purple colour, fcarcely perceptibly different in degree; though I thought the infufion, after ftanding a while, acquired rather a more dufky purple than the decoction.

The infufion had a deeper tinge, and more of the tafte and fmell of the Bark in fubftance than the decoction. Its tafte indeed exactly refembled the Bark, after it had been broke down and chewed for fome time in the mouth.

Expe-

Experiment II.

Equal quantities of each refiduum were boiled in three ounces of fpring water over a flow fire, for the fpace of twenty minutes. The decoctions were equally turbid, exactly fimilar in tafte; and on the admixture of the chalybeate folution in the proportion of one dram to half an ounce, they affumed precifely the fame colour, namely a dufky brown, like chocolate, but inclining fomewhat to purple.

Experiment III.

Five ounces of each refiduum were infufed, for the fpace of forty hours, in an ounce and a half of Jamaica rum, which was fufficiently pure, and unimpregnated with any aftringent matter from the cafk. The tinctures were exactly alike in tafte and colour; and on the addition of one dram of the chalybeate folution, they were inftantly changed from a deep red, to a dark and dirty brown; which was precifely the fame in both tinctures.

Experiment IV.

To half an ounce of powdered Bark, was added an ounce of cold fpring water. The mixture was well triturated in a marble mortar, after which it was fuffered to remain at reft until the grofs powder fubfided. The clear liquor was then carefully poured off, and frefh water to the quantity of half an ounce was added; the trituration was renewed, and afterwards part of the menftruum poured off again as before. This method was purfued for the fpace

of thirty-four hours, in which time ſix ounces of water were combined with the Bark. The mixture was then infuſed fourteen hours without heat; and ſtrained off. This infuſion was found to have the ſmell and taſte of the Bark, in a conſiderable greater degree than either the decoction or infuſion without trituration of Experiment I. and it aſſumed a much blacker colour, on the admixture of one dram of the chalybeate ſolution, than either of the two former preparations.

Experiment V.

It was attempted to determine the comparative ſtrength, or rather aſtringency, of five preparations of the Bark, viz. the extract, decoction, cold infuſion, tincture and triturated infuſion. Ten grains of the extracts carefully made, and as free from empyreuma, as this officinal preparation is generally found to be, were mixed with an ounce of hot water. But ſo imperfect was the ſolution, or, to ſpeak more properly, the ſuſpenſion of the Bark, that in a few minutes a large powder was depoſited at the bottom of the glaſs. This however was ſhaken up, and one dram of the chalybeate ſolution was added to the mixture. The ſame quantity was added to half an ounce of the decoction, infuſion, tincture, (Pharm. Lond.) and triturated infuſion; the laſt aſſumed by far the deepeſt black; the extract approached neareſt to it, and the tincture appeared to be the leaſt tinged. The decoction and infuſion were preciſely alike in colour.

Experiment VI.

The reſiduum of the triturated infuſion, Experiment IV. was boiled over a ſlow fire in three ounces of water, for the ſpace of twenty minutes. The decoction when cold was ſtrained off, it was of a paler colour than the decoctions mentioned Experiment II. although there was a portion of powdered Bark, ſuſpended in it, which, by the trituration, had been rendered fine enough to paſs through the filter. This powder, on ſtanding, ſubſided to the bottom of the veſſel, and left the decoction much more limpid than it was before.

To equal quantities of this, and of the two decoctions mentioned above, one dram of the chalybeate ſolution was added; the black tinge was manifeſtly weakeſt in this decoction, though the difference was not ſo great as might have been expected, from the diverſity in their ſenſible qualities of taſte and ſmell; owing perhaps to the fine powder of the Bark, which floated in it, and retained ſome degree of its original aſtringency.

Experiment VII.

Equal quantities of the ſimple and triturated infuſion were boiled for the ſpace of ſeven minutes over a quick fire; both loſt their tranſparency when cool; but the latter aſſumed a much more turbid appearance than the former, exceeding even that of the decoction from freſh Bark, Experiment I. and, after ſtanding twenty-four hours, it depoſited a very copious ſediment.

Experiment VIII.

Half an ounce of powdered Bark was infused forty-eight hours in five ounces of spring water, and one ounce of white wine vinegar. The mixture was placed near a warm fire, and frequently shaken up. It was then filtered through a linen rag doubled. The taste of the vinegar was in a good measure covered, though the smell was not; but the menstruum was not so fully impregnated with the flavour of the Bark, as the infusion Experiment I. One dram of the chalybeate solution was added to this acid infusion. At first no change of colour took place, but in a few hours a slight black tinge appeared.

Experiment IX.

Half an ounce of powdered Bark was well triturated, in the manner described in Experiment IV. with six ounces of warm water, after which the mixture was poured into a bottle, placed near a fire, and frequently shaken up. This process lasted forty-eight hours. The infusion, when strained off, was found to be more perfectly impregnated with the Bark, than the triturated infusion in cold water, Experiment IV. as appeared by comparing their colour, taste and smell, and by the deeper black which it instantly assumed on the admixture of one dram of the solution of *sal martis*.

Experiment X.

Half an ounce of powdered Bark, and two drams of stone quick-lime, warm from the kiln, were

rubbed together till they were thoroughly united; then 6 ounces of ſpring water were gradually poured on, the powder and the water were well incorporated by triture, and the mixture was ſet by to infuſe for twelve hours. Two ounces of it were then filtered through a double linnen cloth, the remainder ſtood thirty-ſix hours longer; and was frequently ſhaken up, after which it was ſtrained off. The ſmell of the Bark was almoſt entirely covered in both the infuſions, which were ſtrongly impregnated with the lime; and had an extremely diſagreeable flavour. The firſt was of a pale colour, and poſſeſſed but a ſlight degree of bitterneſs; the latter had a deeper tinge, and was equally bitter and nauſeous. Neither of them ſtruck a black colour with the chalybeate ſolution, which, as ſoon as it was added, occaſioned a yellow ſediment, that in a few hours ſubſided to the bottom of the glaſs. Compared with the triturated infuſion Experiment IV. theſe preparations appeared to be much weaker both in colour and taſte. The reſiduum did not ſenſibly efferveſce with oil of vitriol.

Experiment XI.

The decoctions and infuſions were found to be impaired in ſtrength, after ſtanding ſix or ſeven days, though it was the winter ſeaſon, and the weather was ſeverely cold; the infuſions became paler coloured, and at the ſame time depoſited a ſlimy ſediment. The decoction, at the end of ſeven days, aſſumed almoſt a milky hue, and ſtruck but a faint black with the chalybeate ſolution. The ſimple infuſion alſo

had loſt much of its aſtringency; but the two triturated infuſions were very little altered in that reſpect.

Remarks on the preceding Experiments.

PHYSICIANS in general agree, that the Peruvian Bark is moſt powerful in its effects, when taken in ſubſtance. But as the ſtomach is frequently unable to bear it, and as many patients have almoſt an invincible averſion to it in that form; it is of importance to determine in what preparations the virtues of this valuable drug are leaſt impaired; and whether it may not be adminiſtered, under a form that is elegant, palatable, and at the ſame time ſufficiently efficacious. The decoction of the Bark, has always appeared to me an injudicious preparation, for, though the cortex is not a ſubſtance of much volatility, yet there is a certain aroma accompanying it, which the heat of boiling water cannot fail to diſſipate; and conſequently the medicine is deprived of one of its component parts, in which probably ſome of its virtues reſide. The Bark likewiſe undergoes a decompoſition by boiling, the reſin is ſeparated from the gum, and remains ſuſpended in the watery menſtruum. This renders its appearance inelegant, its taſte nauſeous; and, I ſhould apprehend, muſt conſiderably diminiſh its efficacy. For as the virtues of the Bark are ſtrongeſt in its native ſtate, they depend in all probability on its compoſition as a mixt; and muſt of courſe be impaired, by the diſuniting of its conſtituent principles. By the firſt, ſecond, and third, Experiments, it appears, that the

 cortex

cortex yields its virtues at leaſt as perfectly to cold, as to boiling water; and the ſimple infuſion hath certainly many advantages over the decoction. It is a much more agreeable and elegant preparation, and the principles of the Bark remain perfectly unaltered in it, retaining the ſame proportions to each other, as in the ſubſtance of the drug itſelf. Nature hath ſo accurately combined and blended together the gummy and reſinous parts of the cortex, that by their union they become ſoluble in menſtrua, with which when ſeparate they refuſe to unite. Thus they reciprocally promote the ſolution of each other, in water and ardent ſpirits; and both the tincture and infuſion are found, by Experiment, to be ſtrongly impregnated with theſe two conſtituent principles of the Bark. The tincture is, without doubt, an elegant and palatable preparation; but it is liable to this objection, which indeed hold equally true againſt ſpirituous tinctures in general: that a ſufficient doſe of the medicine cannot be taken, on account of the heating nature of its vehicle. In low nervous fevers, hyſterical diſorders, and other caſes where it is neceſſary to join cordials with the Bark, an infuſion of it in red port may be preſcribed with advantage. Under this form the empiric Talbot uſed to adminiſter the cortex, in the paroxyms of intermittents; and ſo ſucceſsful was his practiſe, that Lewis the XIVth was induced to purchaſe, at a large price, the ſecret of his ſpecific. Orange peel is an uſeful ingredient, in preparations of the Bark; it gives a grateful warmth to the infuſion, and adds, I think, conſiderably to its efficacy. The following formula is agreeable to the

taſte,

taſte, and well adapted to a weak and delicate ſtomach.

℞ Pulv. Cort. Peruv. ℥j
Cort. Aurant. inciſ. ℥ſs
Aq. Cinnam. ten. ℥xij
Aq. Cinnam. ſpir. ℥ij M. f.
Infuſio in vaſe clauſo, ſine calore, per ſeſquidiem; deinde coletur.

The uſe of trituration, in promoting the powers of ſolution, is evident from Experiments IV. VI. and VII. and would have been ſtill more ſo, if a proper apparatus had been employed. The Count de la Garaye, a French gentleman, who is diſtinguiſhed for his aſſiduity in applying the different branches of philoſophy to the improvement of medicine, has deſcribed a very convenient machine; and pointed out an admirable proceſs for obtaining from vegetables, by triture with water, the matters in which their virtues chiefly reſide. The contrivance is extremely ſimple, conſiſting only of a veſſel, to which a churning ſtaff is fitted, which, by means of a wheel, and a cord, is perpetually whirled round with a rotatory motion. By this conſtant agitation, the moſt accurate diffuſion is produced; and different portions of the menſtrua are in quick ſucceſſion applied to every part of the ſolvend.

From the Vth Experiment, no certain concluſions can be deduced; except that the extract is a much weaker preparation than is commonly ſuppoſed. It is liable to all the objections, which have been advanced againſt the decoction; with this additional one, that

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it is hardly poſſible to make it, according to the proceſs of the London diſpenſatory, without giving it ſome degree of empyreuma. The extract employed in my experiment was prepared by a very diligent and careful apothecary; yet a conſiderable portion of it preſently ſubſided in a powdery form to the bottom of the glaſs, which on examination appeared to be the burnt parts of the Bark. How little then is this officinal preparation to be depended upon, when we conſider the careleſſneſs and inaccuracy of the generality of apothecaries!

It is the practice of the moſt eminent phyſicians, to join acids with the Bark, in the cure of putrid diſeaſes; and Sir John Pringle has obſerved, that in bilious fevers the cortex anſwered beſt in rheniſh wine, after ſtanding a night in infuſion [a]. This ſuggeſted to me the VIIIth Experiment and I flattered myſelf, that by macerating the Bark in a mixture of vinegar and water, theſe two antiſeptic medicines would be more accurately combined together; and that perhaps the acid might promote the diſſolving power of the aqueous menſtruum. In the latter expectation it appears that I was diſappointed; and whether the former was better grounded, muſt be left to abler judges to determine. The reſult of this Experiment was ſo contrary to my expectations, that I determined to make further trials of the effects of vinegar in deſtroying that property, in certain vegetable ſubſtances, by which they ſtrike a black colour with chalybeates, which has been long regarded as an almoſt indubitable teſt of aſtringency.

[a] Diſeaſes of the Army, Edit. iv. p. 213.

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An ounce of the infusion of chamomile flowers, was divided into two equal parts; to one was added a dram of white wine vinegar, to the other an equal quantity of spring water. Thus, with respect to dilution, they were precisely in the same circumstances; a tea spoonful of the *sal martis*, was then mixed with each of them, the portion which contained the vinegar, suffered no change of colour; the other instantly assumed a dusky hue. The same Experiment was repeated, with a very strong triturated infusion of the Bark; and the result was still more singular and curious. As soon as a dram of vinegar was added to half an ounce of the infusion, it changed the colour of it, from a deep and reddish brown, to a bright yellow; whilst the same quantity of water had no sensible effect on the other portion with which it was mixed. The chalybeate solution, as in the former Experiment, was then added. It produced no alteration in the portion with vinegar, but the other was changed into a perfect ink.

That moderate heat promotes and assists the action of water, as a menstruum on the Bark, is evident from the IXth Experiment; and it would be of advantage to determine, what degree of heat this drug will admit of, without suffering a decomposition. It should however be remarked, that this infusion though stronger, had neither so agreeable a flavour, nor was so sensibly impregnated with the aroma of the Bark, as the two made with cold water.

In an essay on the dissolvent power of quick-lime, a very ingenious chemist has observed, that all resinous bodies become soluble in water, when the cohesion of their particles is destroyed, by withdrawing the

the fixt air which they contain. This method of ſolution he endeavours to apply to many valuable purpoſes in medicine, and hath deſcribed ſeveral uſeful and curious proceſſes for obtaining ſtrong and elegant tinctures of the moſt active drugs, by means of quick-lime. The firſt part of the Xth Experiment, *mutatis mutandis*, was borrowed from him; and it was hoped, that an efficacious and palatable infuſion might with tolerable expedition be made, by the proceſs which he has laid down. But the ſucceſs of my Experiment was by no means anſwerable to the plauſibility and ingenuity of the theory, which induced me to attempt it. The infuſion, after ſtanding twelve hours, the time preſcribed by Mr. Mc'Bride, was but weakly impregnated with the Bark; and when the maceration had been continued forty-eight hours, it by no means equalled in ſtrength the preparation deſcribed Experiment IV. It appears therefore that quick-lime, whatever its effects may be upon other medicines, neither quickens nor increaſes the ſolubility of Bark in water; and it communicates to the infuſion a taſte which is intolerably nauſeous and diſagreeable. That the chalybeate ſolution ſhould produce no change in the colour of theſe preparations, is agreeable to the laws of elective attraction. For the acid of vitriol, having a ſtronger affinity with abſorbent earths than with metallic ſubſtances, forſakes the iron with which it was combined, and unites itſelf to the quick-lime. Hence aroſe the yellow ochry ſediment, taken notice of in the Experiment. As the reſiduum, after filtration, did not efferveſce with oil of vitriol, it is evident that quick-

quick-lime is not endued with the power of abſtracting from Bark the fixed air which it contains.

Experiment X. furniſhes no other inference than this obvious one, that the decoction and infuſion of the Bark are calculated only for immediate uſe. The cortex is a ſubſtance of a very fermentable nature, as appears from the Experiments of Mr. M'Bride; and when its active parts are diffuſed in water, and ſeparated from thoſe which are merely ligneous and inert, it is not to be wondered at, that it undergoes thoſe changes, to which all vegetables, when favourably circumſtanced, are liable.

Warrington,
February 1, 1767.

Thomas Percival.

Received April 24, 1767.

XXVII. *An Inquiry into the probable Parallax, and Magnitude of the fixed Stars, from the Quantity of Light which they afford us, and the particular Circumstances of their Situation, by the Rev.* John Michell, *B. D. F. R. S.*

Read May 7, and 14, 1767.

THOUGH no man can at present doubt, that the want of a sensible parallax in the fixed stars, is owing to their immense distance, yet it may not perhaps be disagreeable to see, that this distance is farther confirmed by other circumstances; for let us suppose them to be, at a medium, equal in magnitude and natural brightness to the sun, to which they seem in all respects to be analagous. And, having laid this down as a foundation to build upon, let us inquire what would be the parallax of the sun, if he were to be removed so far from us, as to make the quantity of the light, which we should then receive from him, no more than equal to that of the fixed stars. In order to do this with accuracy, it would be proper to compare the quantity of light; which we at present receive from him, with that of the fixed stars, by some such methods, as are made use of by Monsieur Bouguer

Bouguer in his *Traité d'Optique* [a]; but as my present purpose does not require any such exactness, I shall deduce it in a more gross way from facts already well known. I shall assume Saturn then in opposition, exclusively of his ring (and when the earth and he are at their mean distances from the Sun) as equal or nearly equal in light to the most luminous fixed star. Now the mean distance of Saturn from the Sun, being equal to about 2082 of the Sun's semidiameters, the density of the Sun's light, at Saturn, will consequently be less than at his own surface, in the proportion of the square of 2082 (or 4334724) to 1. If Saturn therefore was to reflect all the light, that falls upon him, he would be less luminous in the same proportion; but, besides this difference in his brightness, his apparent diameter, in opposition, is at most but 105th part of that of the Sun, and consequently the quantity of light, which we receive from him, must again be diminished in the proportion of the square of 105 (or 11025) to 1. If we multiply these two numbers together, we shall have the whole of the light of the Sun to that of Saturn (upon the supposition of his reflecting all the light, that falls upon him) as the square of nearly 220000 (or 48,400,000,000) to 1; and removing the Sun to 220000 times his present distance, he would still appear at least as bright as Saturn, and his whole parallax upon the diameter of the earth's orbit would be less than two seconds. This must consequently be assumed for the parallax of the brightest of the fixed stars, upon the supposition that their light does not exceed that of Saturn.

[a] This work was published at Paris in 1760.

By a like computation we ſhall find, that the diſtance, at which the Sun would afford us as much light, as we receive from Jupiter, is not leſs than 46000 times his preſent diſtance, and his whole parallax, in that caſe, upon the diameter of the earth's orbit, would not be more than nine ſeconds, the light of Jupiter and Saturn, as ſeen from the Earth, being in the ratio of about 22 to 1, when they are both in oppoſition, and ſuppoſing them to reflect equally in proportion to the whole of the light that falls upon them.

But if Jupiter and Saturn, inſtead of reflecting the whole of the light, that falls upon them, ſhould in fact reflect only a part of it, as for example, only a fourth or fixth (and this may very poſſibly be the caſe), we muſt then increaſe the diſtances computed above, in the proportion of 2 or $2\frac{1}{2}$ to 1, to make the Sun's light no more than equal to theirs; and his parallax would be leſs, in the ſame proportion, than thoſe already mentioned [a].

Upon the ſuppoſition then, that the fixed ſtars are of the ſame magnitude and brightneſs with the Sun, it is no wonder, that their parallax ſhould have hitherto eſcaped obſervation, ſince, if this is the caſe, it could hardly amount to two ſeconds, and probably

[a] The light, which we receive from the full Moon (according to Monſieur Bouguer's experiments in the work above-mentioned), is only a 300000th part of that which we receive from the Sun, whereas it ought to amount to no leſs than a 45000th part of it, according to the principles, which we have made uſe of in computing the quantity of light derived from Jupiter and Saturn; ſo that the Moon, as appears from theſe experiments, reflects no more than between a ſixth and a ſeventh part of the light that falls upon her.

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not more than one in Sirius himſelf; though he had been placed in the pole [b] of the ecliptic, and in thoſe, that appear much leſs luminous, ſuch for example as γ draconis, which is only of the third magnitude, it could hardly be expected to be ſenſible with ſuch inſtruments as have hitherto been uſed in ſearch of it.

We have aſſumed the magnitude of the fixed ſtars, as well as their brightneſs, to be equal to thoſe of the ſun; it is however probable, that there may be a very great difference amongſt them in both theſe reſpects; and how much ſoever we may therefore be wide of the truth, in attempting to fix the diſtance of particular ſtars from this reaſoning, yet there is a very great probability, that their mean diſtances, ſettled by this method, will not be much out, ſome exceeding and ſome falling ſhort of it. And perhaps the conſideration, that a ſtar muſt be a thouſand times as great, *cæteris paribus*, to appear equally bright, if it is placed at ten times the diſtance, may ſerve to make it probable, that the limits of the errors, which we are likely to commit, in judging by ſuch a rule, are not ſo great as we might otherwiſe imagine them to be.

With regard to the difference there may be in the native brightneſs of different ſtars, though it is probably very conſiderable, yet I think we can hardly ſuppoſe, that it is equal to their difference in magnitude, at leaſt if we except thoſe, which are ſubject to certain changes, and which for that reaſon we may ſuppoſe to be luminous in ſome parts of their ſurfaces

[b] The latitude of Sirius being only 39° 33′, his parallax will be a little leſs than two thirds of the whole parallax.

only

only. In other inſtances we may perhaps judge in ſome degree of the native brightneſs of different ſtars with reſpect to one another by their colour; thoſe, which afford the whiteſt light, being probably the moſt luminous [a].

[a] We have at preſent no means of judging of the comparative brightneſs of the Sun and of the fixed ſtars, in proportion to their reſpective ſizes, excepting from the compariſon of the Sun's brightneſs with that of our common fires; but the Sun's light exceeds the light of our brighteſt fires in ſo very great a proportion, (viz. of ſome thouſands to one) that we want ſome middle terms to be able to form any analogy, which might ſerve to carry us farther. We find however in general, that thoſe fires, which produce the whiteſt light, are much the brighteſt, and that the Sun, which produces a whiter light than any fires we commonly make, vaſtly exceeds them all in brightneſs; it is not therefore improbable, from this general analogy, that thoſe ſtars, which exceed the Sun in the whiteneſs of their light, may alſo exceed him in their native brightneſs; now this is the caſe with regard to many of them; and, on the contrary, there are ſome that are of a redder colour.

If however it ſhould hereafter be found, that any of the ſtars have others revolving about them (for no ſatellites ſhining by a borrowed light could poſſibly be viſible), we ſhould then have the means of diſcovering the proportion between the light of the Sun, and the light of thoſe ſtars, relatively to their reſpective quantities of matter; for in this caſe, the times of the revolutions, and the greateſt apparent elongations of thoſe ſtars, that revolved about the others as ſatellites, being known, the relation between the apparent diameters and the denſities of the central ſtars would be given, whatever was their diſtance from us: and the actual quantity of matter which they contained would be known, whenever their diſtance was known, being greater or leſs in the proportion of the cube of that diſtance. Hence, ſuppoſing them to be of the ſame denſity with the Sun, the proportion of the brightneſs of their ſurfaces, compared with that of the Sun, would be known from the compariſon of the whole of the light which we receive from them, with that which we receive from the Sun; but, if they ſhould happen to be either of greater or leſs denſity than the Sun, the whole of their light not being

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As far then as we can guefs at the parallax of the fixed ftars from the principles above laid down, we may reafonably expect, that it fhould be exceedingly fmall even in thofe of the firft magnitude; yet, befides the probability, that fome of them may be either lefs, or lefs luminous than the Sun, it is not fo fmall as to leave us altogether without hopes, that we may fome time or other be able to difcover it in fome of them; for I think it is not impracticable to conftruct inftruments, capable of diftinguifhing even to the 20th part of a fecond, provided the air will admit of that degree of exactnefs; but fuch inftruments muft be upon a plan a good deal different from thofe hitherto made ufe of, as they would otherwife be not only vaftly too expenfive, but alfo much too great and unweildy to be of any ufe.

But whatever room there may be to hope, that we may fome time or other be able to difcover the parallax of a few amongft the fixed ftars, yet at the fame time it feems probable, that we fhall never be able to difcover any fenfible magnitude in their apparent diameters, which in Sirius himfelf, if he is not of lefs native brightnefs than the Sun, muft be confiderably lefs, whatever be his parallax, than the

affected by thefe fuppofitions, their furfaces would indeed be more or lefs luminous, accordingly as they were, upon this account, lefs or greater; but the quantity of light, correfponding to the fame quantity of matter, would ftill remain the fame.

The apparent diftances, at which fatellites would revolve about any ftars, would be equal to the femiannual parallaxes of thofe ftars, feen from planets revolving about the Sun, in the fame periodical times with themfelves, fuppofing the parallaxes to be fuch, as they would be, if the ftars were of the fame fize and denfity with the Sun.

 hundredth,

hundreth, probably than the two hundreth part of a ſecond; ſo that it would ſcarcely be diſtinguiſhable with a teleſcope, upon the former ſuppoſition, that ſhould magnify ſix, or, upon the latter with one, that ſhould magnify twelve thouſand times.

Nor can we well expect to find their apparent diameters from any occultation by the moon, ſince a diameter of the hundredth part of a ſecond would be covered by the moon, if it entered directly, in leſs than the fiftieth part of a ſecond of time, and therefore a ſtar can hardly enter ſo obliquely, as to appear to vaniſh by degrees; no ſtar probably, which the moon can paſs over, ſubtending an angle half ſo great. A ſtar might however appear to vaniſh by degrees in an occultation by the planet Venus, eſpecially if the occultation was to happen only a little before or after either ſtation; but this is an event, which can occur ſo very ſeldom, that little is to be expected from it; and if Venus ſhould be ſurrounded with an atmoſphere, which is probably the caſe, it might very poſſibly then be of no ſervice at all. For the ſame reaſons alſo it is probable, that nothing can be determined from occultations by any of the reſt of the planets, which upon other accounts are ſtill leſs proper for the purpoſe than Venus.

There ſeems to be little chance therefore of diſcovering with certainty the real ſize of any of the fixed ſtars, and we muſt conſequently be contented to deduce it from their parallax (if that ſhould ever be found) and the quantity of light which they afford us, compared with that of the Sun. And in the mean time, till this parallax can be found, or ſomething elſe may ariſe to furniſh us with a more general analogy,

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we can only ſuppoſe them, at a medium, to be equal in ſize to the Sun, this being the beſt means, which we have at preſent of forming ſome probable conjecture concerning the extent of the viſible univerſe. That we may be the better enabled to do this, it ſeems to be an object worth the attention of Aſtronomers, to enquire into the exact quantity of light, which each ſtar affords us ſeparately, when compared with the Sun; that, inſtead of diſtributing them, as has hitherto been done, into a few ill defined claſſes, they may be ranked with preciſion both according to their reſpective brightneſs, and the exact degree of it.

A catalogue of the ſtars formed upon this plan, would anſwer ſeveral good purpoſes; for, beſides giving us a more juſt and certain notion of their general diſtances, it would perhaps help us, eſpecially if the parallax of a few amongſt them ſhould be diſcovered likewiſe, to trace ſome analogies in their ſituation, which might enable us to determine both their diſtances, and other circumſtances relating to them, with ſtill more probability; and it would be a ſtanding regiſter, by which future Aſtronomers might inform themſelves of many variations, of which we are now ignorant for want of an ancient regiſter of that kind.

From what has been ſaid above, I think it is very probable, that we ſhall not be a great way from the truth in eſtimating the whole parallax of Sirius at one ſecond, ſuppoſing him to be of the ſame ſize and native brightneſs with the Sun; this therefore I ſhall aſſume as a ſtandard, till ſome better experiments ſhall inform us more exactly of the quantity of his

 light.

light. Now, according to the beſt judgment I have been able to make from ſome groſs experiments, the quantity of light which we receive from Sirius does not exceed the light which we receive from the leaſt fixed ſtars of the ſixth magnitude in a greater proportion than that of 1000 to 1, nor in a leſs proportion than that of 400 to 1; and the ſmaller ſtars of the ſecond magnitude ſeem to be about a mean proportional between the two. Hence the whole parallax of the leaſt fixed ſtars of the ſixth magnitude, ſuppoſing them of the ſame ſize and native brightneſs with the Sun, ſhould be from about $2'''$ to $3'''$, and their diſtance from about eight to twelve million times that of the Sun: and the parallax of the ſmaller ſtars of the ſecond magnitude, upon the ſame ſuppoſition, ſhould be about $12'''$, and their diſtance about two million times that of the Sun.

I have hitherto argued about the diſtances of the fixed ſtars, upon the ſuppoſition of their being of the ſame ſize and brightneſs with the Sun; and, if this was really ſo, thoſe which appear the brighteſt muſt be the neareſt to us. That this is in general the caſe, I ſuppoſe, will be very readily allowed; for, though it is true, that a much greater degree of real magnitude may compenſate for the greatneſs of diſtance, and there is no reaſon for aſſigning any one limit to them rather than another; yet, when it is as likely that the largeſt ſtars ſhould be in any one part of ſpace as in any other, the probability in favour of this hypotheſis is very great: the real motions alſo, which have been obſerved amongſt ſeveral of the brighteſt of the fixed ſtars, is another argument to the ſame purpoſe; and we ſhall find it ſtill farther

confirmed

confirmed by very ſtrong arguments of analogy drawn from the circumſtances of the particular ſituation of the ſtars in the heavens.

It has always been uſual with Aſtronomers to diſpoſe the fixed ſtars into conſtellations: this has been done for the ſake of remembering and diſtinguiſhing them, and therefore it has in general been done merely arbitrarily, and with this view only; nature herſelf however ſeems to have diſtinguiſhed them into groups. What I mean is, that, from the apparent ſituation of the ſtars in the heavens, there is the higheſt probability, that, either by the original act of the Creator, or in conſequence of ſome general law (ſuch perhaps as gravity) they are collected together in great numbers in ſome parts of ſpace, whileſt in others there are either few or none.

The argument, I intend to make uſe of, in order to prove this, is of that kind, which infers either deſign, or ſome general law, from a general analogy, and the greatneſs of the odds againſt things having been in the preſent ſituation, if it was not owing to ſome ſuch cauſe.

Let us then examine what it is probable would have been the leaſt apparent diſtance of any two or more ſtars, any where in the whole heavens, upon the ſuppoſition that they had been ſcattered by mere chance, as it might happen. Now it is manifeſt, upon this ſuppoſition, that every ſtar being as likely to be in any one ſituation as another, the probability, that any one particular ſtar ſhould happen to be within a certain diſtance (as for example one degree) of any other given ſtar, would be repreſented (according to the common way of computing chances) by a fraction,

 whoſe

whose numerator would be to it's denominator, as a circle of one degree radius, to a circle, whose radius is the diameter of a great circle (this last quantity being equal to the whole surface of the sphere) that is, by the fraction $\frac{\overline{60'}^2}{\overline{6875.5'}^2}$, or, reducing it to a decimal form, .000076154 (that is, about 1 in 13131); and the complement of this to unity, viz. .999923846, or the fraction $\frac{13130}{13131}$, will represent the probability that it would not be so. But, because there is the same chance for any one star to be within the distance of one degree from any given star, as for every other, multiplying this fraction into itself as many times as shall be equivalent to the whole number of stars, of not less brightness than those in question, and putting n for this number, $\overline{.999923846}^n$, or the fraction $\overline{\frac{13130}{13131}}^n$ will represent the probability, that no one of the whole number of stars n would be within one degree from the proposed given star; and the complement of this quantity to unity will represent the probability, that there would be some one star or more, out of the whole number n, within the distance of one degree from the given star. And farther, because the same event is equally likely to happen to any one star as to any other, and therefore any one of the whole number of stars n might as well have been taken for the given star as any other, we must again repeat the last found chance n times, and

consequently

consequently the number $\overline{.999923846}\big|^{n}$, or the fraction $\frac{13130}{13131}\Big|^{n}$ will represent the probability, that no where, in the whole heavens, any two stars, amongst those in question, would be within the distance of one degree from each other; and the complement of this quantity to unity will represent the probability of the contrary.

By a like reasoning, if we would compute the probability, upon the same supposition, that no two stars should be, one within the given distance x, and the other within the given distance z of some one particular star, we must, first, find the probability, that no star, of the whole number of stars n, would be within the distance x from the given star, which will be represented, as before, by the fraction $\frac{\overline{6875.5'}^{2}-xx}{\overline{6875.5'}^{2}}\Big|^{n}$ and, secondly, we must find the probability, that no star, of the whole number of stars n, would be within the distance z from the given star, which, for the same reason, will be represented by the fraction $\frac{\overline{6875.5'}^{2}-zz}{\overline{6875.5'}^{2}}\Big|^{n}$; and the complements of these to unity will represent the respective probabilities of the contrary: but the probability that two events shall both happen, is the product of the respective probabilities of those two events multiplied together; if therefore we multiply the two last mentioned complements together, we shall have the probability, that some two stars would be, one within the distance x, and the other

other within the distance z from the given ſtar; and the complement of this to unity, will be the probability, that it would not be ſo: let us now ſuppoſe $\frac{c}{d}$ to repreſent this laſt quantity, and, becauſe the ſame event may as well happen in reſpect to any one ſtar, as any other, multiplying this quantity into itſelf n times, according to the number of the ſtars, we ſhall have $\overline{\frac{c}{d}}\Big|^{n}$ repreſenting the probability, that no where, in the whole heavens, would be found any two ſtars, one within the diſtance x, and the other within the diſtance z from the ſame ſtar.

If now we compute, according to the principles above laid down, what the probability is, that no two ſtars, in the whole heavens, ſhould have been within ſo ſmall a diſtance from each other, as the two ſtars β Capricorni, to which I ſhall ſuppoſe about 230 ſtars only to be equal in brightneſs, we ſhall find it to be about 80 to 1.

For an example, where more than two ſtars are concerned, we may take the ſix brighteſt of the Pleiades, and, ſuppoſing the whole number of thoſe ſtars, which are equal in ſplendor to the fainteſt of theſe, to be about 1500, we ſhall find the odds to be near 500000 to 1, that no ſix ſtars, out of that number, ſcattered at random, in the whole heavens, would be within ſo ſmall a diſtance from each other, as the Pleiades are[a].

[a] The computations of theſe probabilities are as follow.

The diſtance between the two ſtars β Capricorni is ſomething leſs than $3'\frac{1}{3}$; according to the rule above laid down, therefore, if we ſuppoſe 230 ſtars equal to theſe in brightneſs, the proba-

If,

If, besides these examples that are obvious to the naked eye, we extend the same argument to the smaller

bility, that no two stars of that brightness will be found, any where in the whole heavens, within the distance of $3' \frac{1}{3}$ from each other, will be represented by the fraction $\left.\frac{\overline{6875.5}^2 - \overline{3.33\ \&c.}^2}{\overline{6875.5}^2}\right|^{230\times230}$.

From twice the Log. of 6875.5 [7.6746086] then, subtract twice the Log. of 3.33 &c. [1.0457496] and the remainder 6.6288590 will be the Log. of the number of times, that $\overline{3.33\ \&c.}^2$ is contained in $\overline{6875.5}^2$ viz. 4254603 times, and consequently $\left.\frac{4254602}{4254603}\right|^{230\times230}$ will be equivalent to the former fraction. From the Log. of 4254602, subtract the Log. of 4254603, and the remainder will be—.000000102, the proportional part for an unit in the number 4254603: this multiplied into 230 times 230, or 52900, gives —.0053958, the Log. of the whole quantity, which corresponds to the proportional part for an unit between 80 and 81; this quantity therefore is equivalent to the fraction $\frac{80}{81}$ nearly, the complement of which to unity is $\frac{1}{81}$.

In the Pleiades, the five stars Taygeta, Electra, Merope, Alcyone, and Atlas are respectively at the distances of 11, $19\frac{1}{2}$, $24\frac{1}{2}$, 27, and 49 minutes from the star Maia nearly. From 7.6746086, twice the Log. of 6875.5, then, as before, subtract 2.0827854, twice the Log. of 11 (the number of minutes between Taygeta and Maia) and the remainder 5.5918232 will be the Log. of the number of times, that $\overline{11}^2$ is contained in $\overline{6875.5}^2$ viz. 390682 times; and, consequently a fraction, whose denominator is this number, and whose numerator is this number less by an unit, multiplied into itself 1500 times, will represent the probability, that no star out of 1500, scattered by chance in the whole heavens, would be within the distance of 11 minutes from the star Maia. From the Log. of 390681 therefore subtract the Log. of 390682, as in the former example, and the remainder will be—.000001111, the proportional part for an unit in the number 390682, which multiplied by 1500 will give us—.0016650 for the Log. of the probability sought. In like manner from

stars,

stars, as well those that are collected together in clusters, such for example, as the Præsepe Cancri,

7.6746086 subtract 2.5800692, twice the Log. of 19½ (the number of minutes between Electra and Maia) and the remainder will be 5.0945394, the proportional part for an unit corresponding to the natural number of which will be —.00000349; and 1500 times this quantity, or —.0052350 will be the Log. of the quantity, representing the probability, that no star out of 1500 scattered by chance would be within the distance of 19½ minutes from Maia. If we follow the same rule for the three remaining stars Merope, Alcyone, and Atlas, we shall find the similar Logs. corresponding to these to be —.0076650, —.0100395, and —.0330300 respectively. The natural numbers corresponding to these five Logs. taken in the same order, are .996173, .988018, .982506, .977148, and .926766, which severally express the respective probabilities, that no stars out of 1500 scattered by chance, would be within the same distances, at which the five stars above mentioned are found to be, from Maia. The complements of these quantities to unity .003827, .011982, .017494, .022852, and .073234, which severally express the respective probabilities, on the contrary, that such stars would be found within the distances above specified from the star Maia, must all be multiplied together, to determine the probability, that these events should all take place at the same time. The sum of the Logs. of these numbers is —9 + .1279139 or —8.8720861, which is therefore the Log. of all these numbers multiplied together; and subtracting this number from 0, or, what amounts to the same thing, changing the sign, we shall have the Log. of the number of times that this quantity is contained in unity, that is, about 74488000 times; a fraction, therefore, whose denominator is this number, and its numerator unity, will represent the probability in favour of all these events taking place together; and a fraction, whose denominator is the same number, and its numerator the same number less by an unit, will represent the probability of the contrary. But, as this event might as well have happened to any other star as to Maia, we must multiply this last fraction into itself 1500 times, according to the supposed number of stars, to find the probability, that it should not have happened any where in the whole heavens. Now the proportional part for an unit upon this number is —.0000000005825, which multiplied

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the nebula in the hilt of Perſeus's ſword, &c. as to thoſe ſtars, which appear double, treble, &c. when ſeen through teleſcopes, we ſhall find it ſtill infinitely more concluſive, both in the particular inſtances, and in the general analogy, ariſing from the frequency of them.

We may from hence, therefore, with the higheſt probability conclude (the odds againſt the contrary opinion being many million millions to one) that the ſtars are really collected together in cluſters in ſome places, where they form a kind of ſyſtems, whilſt in others there are either few or none of them, to whatever cauſe this may be owing, whether to their mutual gravitation, or to ſome other law or appointment of the Creator. And the natural concluſion from hence is, that it is highly probable in particular, and next to a certainty in general, that ſuch double ſtars, &c. as appear to conſiſt of two or more ſtars placed very near together, do really conſiſt of ſtars placed near together, and under the influence of ſome general law, whenever the pro-

by 1500 gives us—.000000087377, the proportional part for an unit in ſomewhat more than 496000.

But it muſt be obſerved, that this number is ſmaller than it ought to be upon two accounts; for, in the firſt place, this method of computation gives only the probability, that no five ſtars would be within the diſtance above ſpecified from a fixth, if they occupied the largeſt ſpace, they poſſibly could do, under that limitation; and ſecondly, we have made no allowance upon account of the different magnitudes, which, if it had been attended to, would have given a ſomewhat greater reſult. Theſe conſiderations, however would have made the reaſoning a good deal more intricate; and we have no need to deſcend to minutiæ, a difference in the proportion of 10 to 1 not at all affecting the general concluſion.

bability is very great, that there would not have been any ſuch ſtars ſo near together, if all thoſe, that are not leſs bright than themſelves, had been ſcattered at random through the whole heavens.

After what has been ſaid, it will be natural to inquire, whether, if the ſtars are in general collected into ſyſtems, the Sun does not likewiſe make one of ſome ſyſtem; and which are thoſe, amongſt the fixed ſtars, that belong to the ſame ſyſtem with himſelf. Now, ſuppoſing the ſtars of one ſyſtem to be, in general and at a medium, of the ſame ſize and brightneſs with thoſe of another, the number of ſtars of any one apparent magnitude would bear the ſame proportion to the number of ſtars of any other apparent magnitude, as they would do, in caſe all the ſtars were ſcattered uniformly, and not in ſyſtems, provided the eye was not placed in or near to one of thoſe ſyſtems. And, in this caſe, the brightneſs decreaſing, as the ſquare of the diſtance inverſely, and the ſphere, in which they are included, increaſing, as the cube of the diſtance directly, the number of ſtars of any one degree of brightneſs and upwards, ſhould be, as the cube of the ſquare root of that brightneſs. Suppoſing then the fainteſt of the 2000 brighteſt ſtars to be leſs bright than the fainteſt of the firſt 70, in the proportion of about 30 to 1 (and I think the difference is not leſs than this) this number is ſmaller, than we might have expected, if the Sun was not one of a ſyſtem, in the proportion of 2000 to about 12000 or 1 to 6; but I ſhall lay the leſs ſtreſs upon this argument, for want of a more certain determination of the proportion of light, which we receive from the ſtars of different magnitudes.

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If, however, upon a more accurate examination, it ſhould be found, that the quantity of light above aſſigned is not far from the truth, or if the difference of light ſhould be greater than I have ſuppoſed it to be (in which caſe the argument will be ſtill ſtronger); this will add a conſiderable degree of weight to the other arguments drawn from analogy, in favour of the Sun's making one of a ſyſtem of ſtars.

If we would now inquire, which are probably thoſe ſtars, which compoſe part of the ſame ſyſtem with the Sun; though it will not be poſſible to point them out with certainty, yet there are ſome marks, by which we may, with great probability include ſome and exclude others, whilſt the reſt remain more doubtful. Thoſe ſtars which are found in cluſters and ſurrounded with many others at a ſmall diſtance from them, belong probably to other ſyſtems and not to ours. And thoſe ſtars, which are ſurrounded with nebulæ, are probably only very great ſtars, which, upon account of their ſuperior magnitude, are ſingly viſible, whilſt the others, which compoſe the remaining parts of the ſame ſyſtem, are ſo ſmall as to eſcape our ſight. And thoſe nebulæ, in which we can diſcover either none, or only a few ſtars, even with the aſſiſtance of the beſt teleſcopes, are probably ſyſtems, that are ſtill more diſtant than the reſt.

The Pleiades, as they appear to the naked eye, have been ſhewn above to be probably a ſyſtem by themſelves; and if we examine them ſtill farther by means of teleſcopes, we ſhall find, that they are ſurrounded with ſo large a number of ſmaller ſtars, as to increaſe the odds againſt the contrary opinion many millions to one. Now ſuppoſing the Pleiades

to be in reality a ſyſtem of ſtars, the probability is at leaſt, I ſuppoſe, an hundred to one, that no one amongſt them, of thoſe viſible to the naked eye, belongs to the ſame ſyſtem with the Sun; but that theſe are only ſuch ſtars, as are greater than the reſt. The exact quantity of this probability depends upon the number of ſtars, viſible to the naked eye, belonging to this ſyſtem; the proportion, that the ſpace occupied by the Pleiades bears to the whole heavens; and laſtly, how far the ſituation of any one of the Pleiades falls in with the general analogy of the ſtars compoſing this ſyſtem, if any ſuch general analogy ſhould appear.

As the nebulæ, and ſmaller conſtellations, compoſed of a great number of ſtars, within a ſmall diſtance from one another, belong probably to other ſyſtems; ſo thoſe, which being placed at greater diſtances from each other compoſe the larger conſtellations, and ſuch as have few or no ſmaller ſtars near them, when examined with teleſcopes, belong probably to our own ſyſtem. Moſt of the ſtars of the firſt and ſecond magnitude have this criterion to diſtinguiſh them as belonging to the ſame ſyſtem with the Sun, beſides ſeveral other circumſtances, ſuch as their greater brightneſs; the proper motions [a], that have

[a] The apparent change of ſituation, that has been obſerved amongſt a few of the ſtars, is a ſtrong circumſtance in favour of thoſe ſtars being ſome of the neareſt to us. This apparent change of ſituation may be owing either to the real motion of the ſtars themſelves, or to that of the Sun, or partly to the one, and partly to the other. *As far as it is owing to the latter* (*which it is by no means improbable may in ſome meaſure be the caſe*) it may be conſidered as a kind of ſecular parallax, which, if the annual parallax of a few of the ſtars ſhould ſome time or other be diſcovered, and the quantity and direction of the Sun's motion

6 been

been obſerved amongſt ſome of them; their being more numerous, than we might naturally expect in proportion to the ſmaller ſtars, if they did not compoſe a part of the ſame ſyſtem with ourſelves, &c.

Beſides the brighter ſtars, it is probable there are many of thoſe of the ſmaller magnitudes alſo, that belong to the ſame ſyſtem with the Sun. Now, if this ſhould be the caſe, many of them being only fainter upon account of their leſs real magnitude, we may ſtand the ſame chance of finding a parallax amongſt ſome of theſe, as amongſt the brigheſt ones, provided we pitch upon ſuch as happen to belong to our own ſyſtem: to direct us with ſome probability to theſe, we have the circumſtances above-mentioned of their compoſing larger conſtellations, and their having few ſtars lying very near to them; and perhaps there may be ſtill a little more reaſon to ſuſpect, that thoſe ſtars form a part of the ſame ſyſtem with ourſelves, where, beſides theſe circumſtances, there have been obſerved changes of brightneſs, &c. amongſt ſeveral of them in the ſame neighbourhood, ſuch for inſtance as the changes, which have been obſerved amongſt ſeveral of the ſtars in the conſtellations of the Swan, Caſſiopeia, &c. We may, I think, alſo venture to add to theſe moſt of thoſe ſtars, that are of a redder hue than the reſt, as it is probable that they are a good deal bigger, in proportion to their brightneſs, than the whiter ſtars [ſee the laſt note

ſhould be diſcovered likewiſe, might ſerve to inform us of the diſtances of many of them, which it would be utterly impoſſible to find out by any other means.

but

but two]. Many of them alſo have been obſerved to have a proper motion of their own, which with ſeveral other concurrent circumſtances tends to make it highly probable, that they are ſome of the neareſt to us.

Having thus endeavoured to eſtabliſh the probability, that the Sun is one of a ſyſtem of ſtars, placed by themſelves in this part of the univerſe, I ſhall next inquire into ſome of the conſequences of this hypotheſis. Now if this is the caſe, and we ſuppoſe the whole number of ſtars, which belong to this ſyſtem only (excluding thoſe which belong to others), to amount to about 1000, we ſhall find it to be an even chance, that the parallax of the neareſt amongſt them does not exceed the parallax of one half that number in a greater proportion than that of 9 to 1, ſuppoſing the Earth to be placed in or near the centre of the whole ſyſtem; nor in a greater proportion than that of about 12 to 1, ſuppoſing it to be placed very near the edge of the ſyſtem; for ſuppoſing 1000 points to be placed within a ſphere of any given magnitude, and that they are equally indifferent to every part of that ſphere, if we aſſume any one of theſe points as a centre, we ſhall find, according to the known doctrine of chances, that there is an equal degree of probability whether any one of the reſt ſhall or ſhall not fall within a ſphere, deſcribed about the point aſſumed, of about ſeven ten thouſandths of the ſize of the whole ſphere; but the radius of ſuch a ſphere is about $\frac{8}{90}$ or a little leſs than $\frac{1}{11}$ of the radius of the whole ſphere, that is about $\frac{1}{9}$ of the radius of a ſphere of half the ſize of the greater one; and therefore a ſphere, of about nine times this radius, will

will include half the greater ſphere, if its centre be aſſumed near the centre of the greater ſphere, and a ſphere of twelve times this radius will include half the greater ſphere, if the point be aſſumed almoſt in the ſurface of it.

If we aſſume the ſtars belonging to our own ſyſtem to be about 1000; ſince they are in all reſpects ſimilar to the Sun (excepting perhaps ſuch amongſt them as are liable to frequent changes), and we have nothing to determine us to one magnitude rather than another, we may moſt reaſonably ſuppoſe the magnitude of the Sun to be a medium amongſt the whole number. Upon this ſuppoſition, he will probably rank only with the ſtars of the fourth magnitude; and his light therefore, if he was removed to the medium diſtance of the other ſtars (viz. a diſtance equal to the radius of the ſphere, that would include half the ſtars of our own ſyſtem) would hardly, I think, exceed a 200th part of the light of Sirius; and conſequently, if the parallax of Sirius would be about one ſecond, if he was of the ſame ſize and native brightneſs with the Sun, it will be an equal chance, that the parallax of one half of the ſtars, belonging to this ſyſtem, is not leſs than one ſecond divided by the ſquare root of 200, that is a little more than 4‴; and it will likewiſe be an equal chance, that the parallax of no one amongſt them exceeds between 9 and 12 times that quantity, or a little more than two thirds of a ſecond.

If, inſtead of 1000 ſtars, we ſuppoſe the whole number, belonging to this ſyſtem, to be only about 350, the Sun will then, if he is of a medium ſize amongſt them, rank probably with the ſtars of the third

third magnitude, and his light, at a medium diſtance, upon this hypotheſis, would be, I apprehend, about a 50th part of that of Sirius. And therefore, according to the reaſoning above, we ſhould then find it an equal chance, that the parallax of one half of theſe 350 ſtars would not be leſs than about $8''' \frac{1}{2}$; and there would be the ſame chance, that the parallax of no one amongſt them would be more than between $50'''$ and about $1''$.

In the former ſuppoſition of 1000 ſtars; the apparent magnitude of the Sun, when removed to the medium diſtance; &c. it ſeems not improbable, that the biggeſt ſtar in the ſyſtem may perhaps exceed the Sun, in the proportion of about 1000 to 1; and in the latter ſuppoſition of 350 ſtars, &c. that it may perhaps exceed the Sun, in the proportion of about 120 to 1.

In whatever proportion the diameter of the Sun is greater or leſs than the medium we have taken for it in the ſuppoſitions above, in the ſame proportion will the parallaxes be increaſed or diminiſhed; and in the inverſe triplicate of that proportion muſt their magnitudes be diminiſhed or increaſed.

Let us now examine the circumſtances of the Pleiades; and, aſſuming the reſpective diſtances of the ſtars, compoſing that ſyſtem, from each other to be, at a medium, equal to thoſe of our own, let us ſee what will be the conſequences of this ſuppoſition. Now, if the Pleiades do not extend farther in the direction of a line drawn between the Earth and them, than in a direction at right angles to that line (which, from their compoſing a ſyſtem, we have a right to ſuppoſe they do not), we can hardly allow the mean

diſtance

diſtance of thoſe, that are next to each other, amongſt the ſix ſtars viſible to the naked eye, to be greater than what would ſubtend an angle, if ſeen directly from the earth, of about forty or fifty minutes. And conſequently the diſtance between them and the earth would be about 70 times that diſtance, and their apparent brightneſs, ſeen from thoſe that are next to each other, muſt be about 4900 times as great, as it appears to us; But η of the Pleiades, if I judge rightly, is not fainter than Sirius in a greater proportion than that of about 100 to 1; this ſtar therefore muſt appear brighter to the neareſt of thoſe ſix, which are viſible to the naked eye, than Sirius does to us, in the proportion of about 49 to 1. Let us now ſuppoſe all the ſtars belonging to the Pleiades, as well thoſe diſcoverable with teleſcopes, as thoſe which are viſible to the naked eye, to be contained within a ſphere, whoſe apparent diameter at the earth is two degrees; and conſequently the mean diſtance of them from a ſpectator placed ſomewhere amongſt them, as it might happen, would ſubtend an angle, when ſeen directly from the earth, of about a degree. Since therefore we have ſuppoſed the diſtances of the ſtars of our own ſyſtem to be, at a medium, equal to thoſe of the Pleiades, and conſequently their mean diſtances from the earth to ſubtend at the Pleiades an angle of one degree, we ſhall have the diſtance of the Pleiades about 57 times as great as the mean diſtance of the ſtars of our own ſyſtem, from the earth. Hence, if the biggeſt of the ſtars of our own ſyſtem ſhould be at this mean diſtance from us, and Sirius ſhould be the biggeſt, η of the Pleiades muſt exceed it in the proportion of about 200 to

to 1; for remòving Sirius to 57 times his present distance, his light would then be fainter than it is, in the proportion of 3249 to 1, that is, fainter than η of the Pleiades in the proportion of 32.49 to 1, supposing η of the Pleiades, as above, to afford us a hundredth part of the light of Sirius; but the magnitude of stars, supposing them equally luminous and their distance to be given, is as the cube of the square root of their brightness; and therefore η of the Pleiades, upon this supposition, must be bigger than Sirius in the proportion of the cube of the square root of 32.49 (that is 185) to 1.

But I must observe, that according to general (and, I believe, I may say universal) analogy in all those nebulæ, in which we discover stars bigger than the rest, these stars are placed towards the middle of their respective systems, and, if therefore the same thing obtains with regard to our own system, this will make η of the Pleiades still something greater.

If the distance of the Pleiades is greater than the mean distance of the stars of our own system, in the proportion of 57 to 1, it would be necessary, in order to make stars, of the same real magnitude amongst the Pleiades, equally visible to us with those of our own system, to take in a pencil of rays of a greater diameter, than the pupil of the eye, in the same proportion: this, after proper deductions for the loss of light, could not well be effected by an object lens of less than two feet aperture. Now Dr. Hooke tells us, in his Micrographia, that, with a telescope of twelve feet length, he discovered in the Pleiades 78 stars, and, with longer telescopes, many more; but

if a teleſcope of twelve feet length, the aperture of whoſe object lens was probably leſs than two inches, increaſed the number of viſible ſtars in the Pleiades to 78, we may well ſuppoſe, that with an object lens of two feet diameter, they would amount to more than 1000. What this number would be muſt depend however upon the gradation of real magnitude amongſt the ſtars of that ſyſtem, to which there muſt neceſſarily be ſome limit, and it is not therefore improbable, that obſervations of the increaſe of the number of ſtars amongſt the Pleiades, &c. with teleſcopes of larger apertures, eſpecially if this was carried on to very large ſizes, might ſerve to inform us of many circumſtances, both with regard to this gradation, and perhaps ſome other things, that would enable us to judge with more probability concerning the diſtances, magnitudes, &c. of the ſtars of our own ſyſtem.

If we now imagine a ſpectator amongſt the Pleiades to take a view of this ſyſtem from thence, ſuppoſing the diſtance, as before, 57 times the mean diſtance of our own ſtars, we ſhould appear to him as a nebula, in which there would be no ſtar bright enough to be diſtinguiſhable by the naked eye; and with a teleſcope, the aperture of whoſe object lens was two inches, he would hardly, I ſuppoſe, be able to diſtinguiſh more than half a ſcore ſtars at the utmoſt.

Having hitherto ſuppoſed the diſtances of the ſtars of our own ſyſtem to be the ſame with thoſe of the Pleiades, and examined the appearances according to that hypotheſis, let us now, inſtead of their diſtances, ſuppoſe their magnitudes to be the ſame. This would make this ſyſtem, as ſeen from the Pleiades, to ſubtend an angle of about twelve degrees inſtead of two,

and about half a ſcore of the biggeſt ſtars would be there viſible to the naked eye; but a teleſcope, whoſe object lens was of two inches aperture, would in that caſe, I apprehend, take in almoſt all the ſtars belonging to this ſyſtem of the fourth magnitude and upwards. Theſe appearances fall in leſs, I think, with the general analogy of what we ſee in the heavens, than the former ſuppoſition; but for want of more obſervations I cannot ſay this with any certainty: in the mean time however till we have ſomething farther to go upon, it may not perhaps be amiſs to take a kind of medium between the two, and ſuppoſe the Pleiades to be at about twenty times the mean diſtance of the ſtars belonging to our own ſyſtem, in which caſe η will exceed the biggeſt of theſe, in the proportion of about eight or ten to one; or it will exceed the Sun, according to our former ſuppoſitions of his being of a medium ſize amongſt 1000 or 350 ſtars, in one caſe in the proportion of about eight or ten thouſand, and in the other, about a thouſand or twelve hundred to one; its parallax in the former caſe being about $36''''$ and in the latter about $1\frac{1}{4}'''$.

I ſhall conclude this inquiry with one obſervation more, concerning the appearance of the ſtars of our own ſyſtem, as ſeen from great diſtances. Whatever then the real diſtance and magnitude of theſe ſtars may be, provided we have not been greatly out in aſſigning the proportion of their light in reſpect to that of the Sun and one another, if they were to be ſeen from a diſtance, at which the whole ſyſtem would not ſubtend an angle of more than ſix or eight minutes, it would appear only as a nebula, no ſingle ſtar being viſible with perhaps any teleſcope, that has ever yet

been made; for at this diſtance, the diſtance between the earth and the biggeſt ſtar of this ſyſtem not ſubtending an angle of more than about three minutes (that is, about a twelve hundredth part of the radius) the ſtars of this ſyſtem muſt appear leſs luminous than they do to ourſelves, in the proportion of the ſquare of 1200 (or 1440000) to 1. And ſuppoſing the light of Sirius to exceed that of the leaſt viſible ſtar in the proportion of about 1200 to 1, the brighteſt ſtar therefore would ſtill require to have its light increaſed in the proportion of 1200 to 1, before it would begin to be diſtinguiſhable. To do this would require a teleſcope, that ſhould take in a pencil of rays of a larger diameter than the pupil of the eye, in the proportion of 35 to 1, that is, a pencil of about a foot diameter, excluſive of deductions; for the pupil of the eye is not leſs than a third of an inch in diameter, in a clear ſtar-light night, when there is no Moon; but to obtain ſuch a pencil, we muſt not make uſe of a refracting teleſcope (with two lenſes only) of leſs than 15 inches, nor a reflector of leſs than nearly two feet aperture. This may ſerve to ſhew us, that thoſe nebulæ, in which we cannot diſtinguiſh any ſtars, may yet reaſonably be ſuppoſed to conſiſt of ſtars, though too far diſtant to be ſingly viſible; ſince this would be the caſe with our own ſyſtem, ſeen from as great a diſtance as we may well ſuppoſe thoſe nebulæ to be from us, if we judge of it from the magnitude of the viſible area, which they occupy in the heavens.

Of the twinkling of the fixed ſtars.

Having never yet ſeen any ſolution of the twinkling of the fixed ſtars, with which I could reſt ſatisfied,

fied [a], I ſhall offer the following, which may not perhaps be found an inadequate cauſe of that appearance; at leaſt it has undoubtedly ſome ſhare in producing it, eſpecially in the ſmaller ſtars.

It is not, I think, unreaſonable to ſuppoſe, that a ſingle particle of light is ſufficient to make a ſenſible impreſſion upon the organs of ſight. Upon this ſuppoſition, a very few particles of light, arriving at the eye in a ſecond of time, will be ſufficient to make an object viſible, perhaps not more than three or four; for though the impreſſion may be conſidered as momentary, yet the perception, occaſioned by it, is of a much longer duration: this ſufficiently appears from the well-known experiment of a lighted body whirled round in a circle, which needs not make many revolutions in a ſecond, to appear as one continued ring of fire. Hence then it is not improbable, that the number of the particles of light, which enter the eye in a ſecond of time even from Sirius himſelf, may not exceed three or four thouſand; and from ſtars of the ſecond magnitude, they may therefore probably not much exceed an hundred. Now the apparent increaſe and diminution of the light, which we obſerve in the twinkling of the ſtars, ſeems to be repeated at not very unequal intervals, perhaps about four or five times in a ſecond: why may we not then ſuppoſe, that the in-

[a] Some aſtronomers have lately adopted, as a ſolution of this appearance, the extreme minuteneſs of the apparent diameters of the fixed ſtars, which, they ſuppoſe, muſt, in conſequence of this, be intercepted by every little mote, that floats in the air; but, that an object ſhould be able to intercept a ſtar from us, it muſt be large enough to exceed the apparent diameter of the ſtar by the diameter of the pupil of the eye; ſo that, if the ſtar was a mathematical point, it muſt ſtill be equal in ſize to the pupil of the eye.

equalities,

equalities, which will naturally arise from the chance of the rays coming sometimes a little denser and sometimes a little rarer, in so small a number of them as must fall upon the eye in the fourth or fifth part of a second, may be sufficient to account for this appearance? An addition of two or three particles of light, or perhaps of a single one upon twenty, especially if there should be an equal deficiency out of the next twenty, would I suppose be very sensible: this seems at least probable from the very great difference in the appearance of stars, whose light is much less different than, I imagine, people are in general aware of; the light of the middle-most star in the tail of the great Bear does not, I think, exceed the light of the very small star next to it, in a greater proportion than that of about sixteen or twenty to one; and Monsieur Bouger tells us, in his Traité d'Optique before-mentioned, that he finds a difference in the light of objects of one part in sixty-six sufficiently distinguishable.

It will perhaps be objected, that the rays coming from Sirius are too numerous to admit of a sufficient inequality, arising from the common effect of chance, so frequently as would be necessary to produce this effect, whatever might happen in respect to the smaller stars; but till we know what inequality is necessary to produce this effect, we can only guess at it either one way or the other; there is however another circumstance, that seems to concur in the twinkling of the stars, besides their brightness, and this is a change of colour. Now the red and blue rays being very much fewer, I apprehend, than those of the intermediate colours, and therefore much more liable to inequality from the common effect of chance, may help very much to account for this phænomenon, a small excess

 or

or defect in either of these making a very sensible difference in the colour.

It will now naturally be asked, why the frequency of the changes of brightness should not be often much greater, as well as sometimes less, than that above-mentioned, and why the interval of the fourth or fifth, or some such part, should be pitched upon, rather than the fortieth or fiftieth part of a second, or than a whole second, &c. for, according to the length or shortness of the time assumed, the changes, that will naturally occur, from the effect of chance, will be smaller or greater in proportion to each other. The answer to this question will, I think, tend to render the above solution more probable, as well as to throw a good deal of light upon the whole subject. The lengths of the times then between the changes of brightness, if I am not mistaken, depend upon the duration of the perception before mentioned, occasioned by the impression of the light upon the eye, than which they seem to be neither much longer nor shorter. Whatever inequalities fall within a much shorter time than the continuance of this perception, will necessarily be blended together, and have no effect, but as they compose a part of the whole mass; but those inequalities, which fall in such a manner as that they may be assigned to intervals nearly equal to, or something greater than the continuance of this perception, will be so divided by the imagination, which will naturally follow, and pick them out as they arise.

END OF PART I.

PHILOSOPHICAL TRANSACTIONS.

VOLUME LVII. PART II.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LVII. PART II. For the Year 1767.

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M.DCC.LXVIII.

PHILOSOPHICAL TRANSACTIONS.

PART II.

Received April 9, 1767.

XXVIII. *Thermometrical Obſervations at* Derby, *by Mr.* John Whitehurſt, *communicated by* Charles Morton, *M. D. Sec. R. S.*

Dear Sir,

Read May 14, 1767.

WE experienced a much greater degree of cold at Derby, in the late froſt, than perhaps was ever obſerved in England; and the quick tranſitions were no leſs remarkable.

On Sunday the 18th of laſt month, at nine of the clock in the evening, my thermometer ſtood at 20. At half an hour after nine, nearly one degrew below o. At ſeven the next morning, 30. external air.

I am, Sir,

Your moſt obedient ſervant,

Derby,
Feb. 15, 1767.

John Whitehurſt.

XXIX. *An Attempt to interpret the Legend and Inſcription of a very curious* Phœnician *Medal, never hitherto explained. In a Letter to the Right Honourable the Earl of* Morton, *Preſident of the Royal Society, from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

My Lord,

Read May 21, 1767.

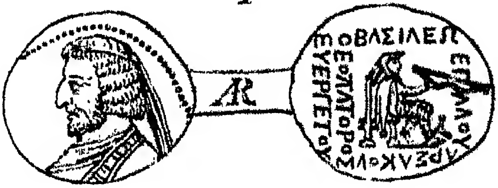
CAPT. Swinton ſome time ſince put into my hands a very curious ſilver coin, taken, as he informed me, out of your Lordſhip's valuable cabinet. He at the ſame time alſo imparted to me your Lordſhip's commands, relative to that coin. In obedience to which, I now do myſelf the honour to ſend you the following interpretation of the legend and inſcription it exhibits. This, as I cannot help believing it true, or at leaſt not very remote from truth, may poſſibly, I would flatter myſelf, prove not altogether unſatisfactory or unacceptable to your Lordſhip.

I.

On one ſide (ſee TAB. XII.) the medal preſents to our view Jupiter ſitting in a chair, with his eagle before him, a bunch of grapes in his right hand, and a ſort of lance, or rather ſtaff, as it ſhould ſeem, in his left. Behind him the legend בעל תרז, BAAL TARZ, or BAAL TARS, formed of Phœnician letters, may be diſcerned;

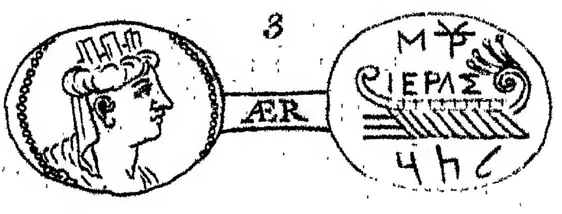
4 and

1



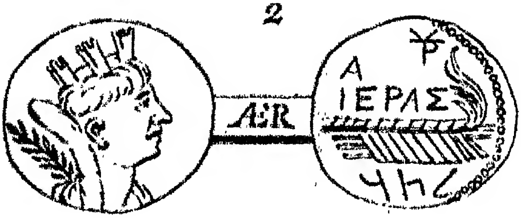
Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

3



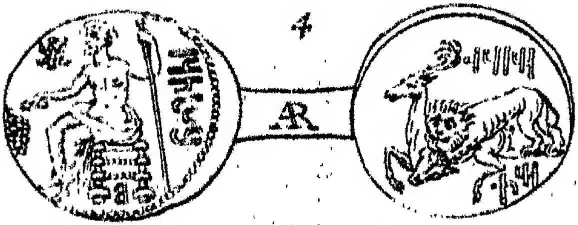
Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

2



Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

4



È Thesauro Illustrissimi Comitis de Morton, R.S.P.

and the element B, inverted, is viſible under the chair. On the other ſide we diſcover a lion ſeizing upon, or rather tearing, a ſtag; over and under which the two Phœnician words מזרג מות, MIZZE-RAG MOTH, or MUTH, in their proper characters, unleſs I am greatly miſtaken, ſeem clearly to appear. The workmanſhip of all the figures, but particularly of the lion and the ſtag, is finiſhed in a high manner, and exquiſitely fine.

Several (1) ſimilar medals have been publiſhed by Lord Pembroke, M. Morell, and M. Pellerin. Amongſt theſe we find four in Lord Pembroke's noble cabinet, emitted from the ſame mint, and done in the ſame taſte, with that I am here conſidering, three of (2) which exhibit a lion tearing a bull. Not one of the legends, or inſcriptions, with which they are adorned, has been hitherto explained. A proper and accurate draught of the medal before me has never yet, as I apprehend, been given us, by any antiquary, or author whatſoever, and therefore the piece itſelf may paſs for an inedited coin.

II.

That the legend of this medal is formed of the letters *Beth*, *Ain*, *Lamed*, *Thau*, *Reſch*, and *Zain*, from which reſult the words BAAL TARZ, or BAAL TARS, will, I flatter myſelf, not be denied by the learned. The firſt character ſtrongly reſembles the

(1) *Numiſm. Antiqu.* &c. à Thom. Pembr. et Mont. Gomer. Com. *Collect.* P. 2. T. 87. Morel. *Specim. Univerſ. Rei Nummar. Antiqu.* &c. P. 245. Tab. xxvi. Lipſiæ, 1695. Peller. *Recueil de Medailles de Peuples et de Villes*, &c. Tom. Trois. Pl. CXXII. A Paris, 1763.

(2) *Numiſm. Antiqu.* à Thom. Pembr. et Mont. Gomer. Com. *Collect.* ubi ſup.

 Beth

Beth of the (3) Palmyrenes, which had moſt certainly the ſame origin with the Phœnician *Beth*. Nor can it be taken here for any other element. Beſides, the common Phœnician form of that letter and the character in queſtion are (4) promiſcuouſly uſed on this ſpecies of coins. The ſecond element is *Ain*, as appears from other (5) authentic monuments. The third is *Lamed*, though not ſo well preſerved here as on ſome other ſimilar (6) coins. The fourth, at firſt ſight, will be pronounced *Thau*, by every one at all verſed in this branch of literature. That the fifth is *Reſch*, we may fairly preſume; as the ſimilar letter *Daleth* appears (7) open at the top, like this, on a medal of Sidon, publiſhed by M. Pellerin, and as ſuch a figure of *Reſch* actually preſents itſelf to our view on the reverſes of two of my Tyrian (8) coins. Nor does the correſpondent character on another of theſe Phœnician medals, a draught of which has been given (9) us by M. Morell, recede much from the common Phœnician form of *Reſch*, which on theſe pieces likewiſe ſometimes occurs. The ſixth is *Zain*, as has been (10) rendered elſewhere ſufficiently evident. What the inverted B, under the chair, was intended to denote, unleſs we may conſider it as the initial letter

(3) *Philoſ. Tranſact.* Vol. XLVIII. p. 693, 740.
(4) Peller. ubi ſup. Pl. CXXII. p. 164.
(5) *Philoſ. Tranſ.* Vol. LIII. p. 282. & Vol. LIV. Tab. xxiv. Peller. *Recueil de Medail. de Peup. et de Vil. &c.* Tom. Trois. p. 164. Pl. CXXII. A Paris, 1763.
(6) Id. ibid.
(7) Peller. ubi ſup. Tom. Sec. Pl. LXXXII. N. 21. p. 226.
(8) See Tab. XII. N. 2, 3.
(9) Morel. ubi ſup. Peller. Tom. Trois. Pl. CXXII.
(10) *Philoſ. Tranſ.* Vol. XLVIII. p. 715. & Vol. LIII. p. 283.

of

of the firſt word BAAL, it may perhaps be not ſo eaſy to decide.

From the two Phœnician proper names BAAL TARZ, or BAAL TARS, it ſeems highly probable, that the medal was ſtruck at Tarſus, the capital of Cilicia, ſeated in a country abounding with wild beaſts, (11) particularly lions and ſtags, and famous for the birth of the great apoſtle St. Paul. For BAAL TARZ, or BAAL TARS, is equivalent to JUPITER TARSENSIS, JUPITER OF TARSUS, or THE LORD OF TARSUS. So we find this Pagan divinity denominated (12) on coins ΖΕΥϹ ΚΑϹΙΟϹ, JUPITER CASIUS, ΖΕΥϹ ΚΑΠΙΤΩΛΙΕΥϹ ΑΝΤΙΟΧΕΩΝ, JUPITER CAPITOLINUS ANTIOCHENSIUM, ΖΕΥϹ ϹΤΡΑΤΗΓΟϹ ΑΜΑϹΤΡΙΑΝΩΝ, JUPITER IMPERATOR AMASTRIANORUM, &c. And, upon (13) a coin of the emperor Hadrian, we read ΔΙΟϹ ΤΑΡϹΕΩΝ ΜΕΤΡΟΠΟΛΕΩϹ, JOVIS TARSENSIUM METROPOLEOS, or JOVIS TARSENSIS, OF JUPITER OF TARSUS, the very appellation given this pretended deity by the medal now in view. Nay, he is there exhibited with his eagle, and ſitting in a chair, as he appears upon the Phœnician coin I am endeavouring to explain. The notion here advanced is alſo ſtrongly ſupported by two pieces of the emperor Gordian, publiſhed by Dr. Vaillant. One of theſe on the reverſe preſents to our view Bacchus, with a

(11) Ælian. *de Animal.* Lib. V. c. lvi. Boch. Chan. Lib. I. c. v. p. 376, 377. Francof. ad Mœn. 1681.

(12) Joan. Vaill. *Numiſm. Imp. Auguſt. & Cæſar. à Pop. Rom. Dit. Græcè loquent. &c.* paſſ. In like manner the great deity of Berytus went under the appellation of BAAL BERITH, according to Bochart. Boch. ubi ſup. p. 859.

(13) Joan. Vaill. ubi ſup. p. 36. Amſtelædami, 1700.

bunch

bunch of grapes (14) in his right hand, as Jupiter appears on the medal before me, attended by the inscription ΤΑΡΣΟΥ ΜΕΤΡΟΠΟΛΕΩΣ, &c. Tarsi metropoleos, &c. and the other is adorned with the same inscription, and a lion tearing a (15) bull, the very symbol that occurs on three of Lord Pembroke's (16) coins, so perfectly similar to that which is the object of my attention here.

In farther eviction of the point I am contending for, I must beg leave to remark, that two pieces of Tarsus with Jupiter Tarsensis on the reverse, in the very same attitude, and, in other respects, resembling the figure of the same deity on the medal now considered, have been lately (17) published by M. Pellerin.

From the coin of Gordian (18) adorned with Bacchus on the reverse, holding a bunch of grapes in his right hand, we may infer, not only that Bacchus was worshiped at Tarsus, but likewise that wine was made in the district appertaining to that city. This will easily and naturally account for the grapes held by Jupiter, or Baal, in his right hand, on the piece I am attempting to illustrate here.

It must be farther remarked, that such medals as this, as well as many others, are dug up in the neighbourhood (19) of Kepse, the Seleucia Pieria of the antients, which stood at no very great distance from

(14) Joan. Vaill. ubi sup. p. 157.
(15) Id. Ibid.
(16) *Numism. Antiqu. &c.* à Thom. Pembr. et Mont. Gomer. Com. *Collect*, p. 2. T. 87.
(17) Peller. ubi sup. Tom. Sec. Pl. LXXIV. p. 175—177. A Paris, 1763.
(18) Joan. Vaill. ubi sup. p. 157.
(19) Pococke's *Descript. of the East*, Vol. II. p. 186.

I mount

mount Amanus and the borders of Cilicia. This, in conjunction with what has been already offered in support of my present opinion, will amount to the strongest presumption, if not an irrefragable proof, that the piece in question was struck at Tarsus. The late Mr. Arthur Pullinger shewed me two similar coins, found near the place just mentioned, which he purchased at Aleppo, and brought with him to England out of the East.

That the Phœnician name TARZ should have answered to the Greek ΤΑΡΣΟΣ, and the Latin TARSUS, can be no matter of surprize to any person at all versed in oriental literature. For 'tis too well known to stand in need of a proof, that ΟΣ is a pure Greek termination, and VS a Latin one, superadded to the Phœnician word. And that the Greeks (20) not infrequently converted *Zain* into *Sigma*, as the Latins did into S, is a most obvious truth. Many instances of such conversions, in oriental names adopted by those nations, might, with the utmost facility, be produced.

III.

With regard to the characters on the reverse, their powers seem sufficiently deducible from other monuments heretofore explained. The first and fifth will undoubtedly be taken for *Mem*. The second points plainly at *Zain*. The third will be looked upon either as *Daleth*, *Caph*, or *Resch*; though, if the sense be duly attended to, I think we must pro-

(20) Boch. Chan. Lib. I. c. xxvii. p. 559. c. vi. p. 390. & alib. Francof. ad Mœn. 1681. The Arabs, after the conquest of Syria, adopted the Greek, or Latin, name; which is evidently not so antient as the Phœnician תרז, TARZ, or TARS.

nounce

nounce it *Resch*. The fourth will be allowed to represent *Ghimel*, as that element appears in some of the (21) Palmyrene inscriptions, and particularly that found (22) at Teive. The sixth, as I apprehend, must pass for *Thau*. Now the words formed of these letters, מזרג מרת, MIZZERAG MOTH, or MISSERAG MUTH, may be rendered AB IMPLECTENDO (COLLIGANDO, CONNECTENDO, &c.) MORS, or rather A CONNECTIONE (COLLIGATIONE, IMPLEXU, CONGRESSU, &c.) MORS, FROM THE CONNECTION (SEIZURE, ATTACK, &c.) comes DEATH. In Hebrew the second letter of the first word here is *Sin*, in Chaldee *Samech*, and in Arabic *Ze*. That in all these languages, however, the term is nearly the same, and has nearly the same signification annexed to it, we learn (23) from some of the best lexicographers.

Nor can any thing be more consonant to the nature of the symbol on the reverse than the sense here assigned the inscription. They may both perhaps be supposed to have alluded to the Indian manner of hunting stags with lions, mentioned by Ælian (24), which might formerly likewise have prevailed in Cilicia; or to the production of those (25) animals, which were anciently so numerous in that country; or lastly to some remarkable event, that not impro-

(21) *Philosoph. Transact.* Vol. XLVIII. p. 693, 740.
(22) See Vol. LVI. Tab. I. Fig. 1. p. 5.
(23) Val. Schind. Johan. Buxtorf. F. *Lex. Chald. Talm. & Rabbin.* Leonh. Reckenb. & Jo. Christ. Clod. *Lex. Hebr. Select.* Lipsiæ, 1744.
(24) Ælian. *de Animal.* Lib. XVII. c. xxvi. p. 990. Coloniæ Allobrogum, 1616.
(25) Ælian. *de Animal.* Lib. V. c. lvi. Boch. Chan. Lib. I. c. v. p. 376, 377. Francofurti ad Mœnum, 1681.

 bably

bably happened about the time the medal was ſtruck.

IV.

The laſt remark naturally leads me to the age of the coin I am endeavouring to explain, to which perhaps a pretty near approach may be made. I formerly publiſhed a ſimilar (26) medal, that, unleſs I am greatly deceived, firſt appeared in ſome part of the Parthian territories. It preſented to our view on one ſide Jupiter Tarſenſis, as he is exhibited by the piece before me, and two words perfectly agreeing with thoſe attending him on this piece, being formed of the very ſame Phœnician characters. On the other we perceived a human figure ſitting in a chair, with an arrow in both its hands, before which ſtood a bow, a weapon not ſeldom occurring on the reverſes of the Parthian coins (27). Hence it ſhould ſeem,

(26) *Philoſoph. Tranſact.* Vol. LI. Tab. xvii. p. 683.

The Parthian kings ſometimes wore a ſort of long breeches, reaching down to their ancles, ſimilar to what is exhibited by the coin here referred to, as we learn from the reverſe of a Parthian medal now in my poſſeſſion, a draught of which may be ſeen in (*a*) Tab. XII. N. 1. This will ſerve ſtill farther to evince the truth of the point here contended for.

That the coin by me formerly publiſhed ought to be ranked among thoſe ſtruck in the Parthian territories, will be rendered ſtill more probable by certain human figures that occur amongſt the ruins of Perſepolis; which are (*b*) adorned with a ſort of cap, not a little ſimilar to that on the head of the prince preſented to our view by this coin. The bow and arrow likewiſe on it, (*c*) viſible alſo on a very curious Daric, will confirm what has been here advanced, in ſupport of my preſent opinion.

(*a*) *Philoſ. Tranſact.* Vol. LVII. Tab. XII. N. 1. p. 266.
(*b*) Engel. Kæmpfer. *Amœnitat. Exotic.* &c. p. 311. Lemgoviæ, 1712.
(*c*) Hyde *Hiſt. Relig. Veter. Perſar.* &c. p. 113. *Oxon.* 1760.

 that

that these pieces were struck at Tarsus, when the Parthians (28) were masters of Cilicia. Now I cannot learn from history, that they were ever possessed of that province before the year of the Julian Period 4673, which preceded about forty one years the birth of CHRIST, when (29) it was reduced by Labienus; nor after the following year, when they were overthrown, and expelled the Roman territories (30), by Ventidius. Perhaps

(28) That the piece before me is of Parthian original, seems likewise farther to appear from a complex figure representing a lion tearing (*a*) a bull, the very type on the reverses of several coins, mentioned in the beginning of this paper, and agreeing in almost all respects with this piece, to be met with in the ruins of (*a*) Persepolis. For that part of those ruins, at least, are Parthian remains of antiquity, I have (*b*) formerly observed. But should any one consider them as Persian monuments, and even as prior to the reign of Alexander the Great, yet still the point I would in some measure support by them will thereby be rendered probable, if not incontestable. For the Persians and the Parthians may be looked upon, with sufficient propriety, as one and the same nation; the same habits, the same customs and manners, and the same religion, seeming antiently to have prevailed amongst them.

Whenever therefore we find a lion tearing a bull, or a stag, on any Greek medals, we may conclude it not improbable, that the prince or people to whom they belonged adopted a type of Parthian, or Persian, extraction.

(*c*) Gesner assigns the coin I am considering a place amongst those of the Syrian kings, but knows not what to make of it. He only observes, that Morell takes it to be a Parthian medal, and that others entertain a different opinion; not attempting, in any part of his book, to oblige the learned world with an explication of it.

(29) Dio, Lib. xlviii. L. Flor. Lib. IV. c. ix. Plutarch. in *Anton.* Appian. Alexandrin. in *Syriac.* & *Parthic.* & *de Bell. Civil.* Lib. v.

(30) Dio, ubi sup. Appian. in *Parthic.* Liv. *Epit.* Lib. cxxvii. L. Flor. Lib. IV. c. ix. Plutarch. in *Anton.*

(*a*) Eng. Kæmpf. *Amœnitat. Exotic.* p. 339. Lemgoviæ, 1712.
(*b*) *Philos. Transact.* Vol. XLIX. P. i. p. 598, 599, 600.
(*c*) Gesn. Tab. *Reg. Syr.* IX. N. 2. Tiguri, 1738.

therefore

therefore we may be permitted to ſuppoſe our medal to have been emitted from the mint at Tarſus in one of thoſe years. If this ſhould be admitted by the learned, and I can ſee nothing unreaſonable in ſuch a ſuppoſition, we may naturally enough account both for the ſymbol and the inſcription. For the Parthians at this time had the Romans in ſuch contempt, by reaſon of their former victories over them, that a body of their forces advancing to the relief (31) of Labienus, then incamped on Mount Taurus, at no very great diſtance from them, engaged a Roman army, before their junction with that general. Being therefore overthrown in this battle by Ventidius, (32) who commanded the Romans, and moſt of them cut in pieces, they received the reward juſtly due to their temerity and preſumption. This certainly gives us reaſon to believe, that the ſymbol and inſcription I am now upon pointed at the victories gained by the Parthians over the Romans, about the time above mentioned; and that they were intended to perpetuate the memory of thoſe victories, to the remoteſt periods of time.

In farther ſupport of what has been here advanced, it may not be improper to obſerve, that a ſimilar (33) medal publiſhed by M. Pellerin ſeems to me to be undoubtedly Parthian. On one ſide we diſcover Hercules, with a club in his right hand, and a lion's ſkin in his left, as he appears on a coin of Tarſus (34), together with a bow, a ſymbol not

(31) Dio, ubi ſup.
(32) Idem ibid.
(33) Peller. ubi ſup. Tom. Troiſ. Pl. CXXII. 11.
(34) Joan. Vaill. ubi ſup. p. 157.

 ſeldom

ſeldom viſible on the Parthian coins. The reverſe agrees with that of the piece I am conſidering, in every particular but the inſcription; from which, tho' Phœnician, that of the medal before me is intirely different. That on M. Pellerin's piece, however, is formed of ſix letters; the three firſt of which are apparently *Mem*, *Lamed*, *Caph*, which conſtitute the word מלך, MELEC, KING. The fourth greatly reſembles the Chaldee *Vau*, and not a little one of the forms of the ſame element (35) uſed by the Palmyrenes. The fifth muſt be taken, as I apprehend, for *Reſch*, being very like the figure of that letter on the medal engaging my attention here. The ſixth may paſs for a form of *Daleth*, that has ſuffered from the injuries of time. Admit this, and I ſee not the leaſt impropriety in ſuch a conceſſion, and the word may be read OROD; which, ſtripped of the Greek termination, is not only a Parthian proper name, but the very name of the prince who ſat upon the throne when (36) the Parthians were poſſeſſed of Cilicia, and their forces overthrown by Ventidius. Had it not been for the ſuppreſſion of the latter *Vau*, this word would have been intirely the ſame with VOROD, or OROD, exhibited by three Palmyrene (37) inſcriptions, and written ΥΟΡΟΔΗΣ, VORODES, or ORODES, in the correſpondent Greek ones. And with regard to the ſuppreſſion of *Vau*, this was by

(35) *Philoſoph. Tranſact.* Vol. XLVIII. p. 693. & Tab. XXX. Num. 3.

(36) Appian. Alexandrin. & Dio, ubi ſup.

(37) *Philoſoph. Tranſact.* Vol. XLVIII. p. 694, 695.

no

no means uncommon amongſt the Phœnicians, as I have elſewhere (38) inconteſtably proved.

If the interpretation of this ſhort inſcription now laid down ſhould meet with the approbation of the learned, it would bring no ſmall acceſſion of ſtrength to the notion here advanced. It would alſo evince, in conjunction with what has been offered, beyond the poſſibility of a doubt, that inſcriptions of very different kinds have been handed down to us by this ſpecies of coins.

That medals ſhould have been emitted from the mint at Tarſus with Phœnician letters upon them, to thoſe verſed in this branch of literature can be no matter of ſurprize. It has been proved, that ſuch pieces as that now in view were (39) ſtruck by the Phœnicians, ſome of whom were undoubtedly ſettled at Tarſus, and conſequently ought to be deemed Phœnician, not Punic, coins.

As the medal itſelf, according to the gentleman who purchaſed it in the Eaſt, ſeems to have been found either in the neighbourhood of Hems, formerly denominated Emeſa (40), or ſomewhere near Eſbele, or Gibele, the Byblus (41) of the antients, and conſequently at no very great diſtance from the borders of Cilicia; my explication of it may poſſibly, I would flatter myſelf, prove not altogether unſatisfactory to the learned.

To what has been here advanced ſome may perhaps object, that we can diſcover little of the Par-

(38) *Philoſoph. Tranſact.* Vol. LIV. p. 419.
(39) Peller. ubi ſup. Tom. Troiſ. p. 162, 163.
(40) Poc. *Obſervat. on Syria*, p. 98.
(41) Id. ibid.

thian

thian taſte or genius in the coins I have been endeavouring to throw ſome light upon. They may tell us that ſuch pieces ſeem much more to reſemble the medals of Syria and Phœnicia than the Parthian coins. To which I would only beg leave to reply, that I conſider them rather as pieces ſtruck at Tarſus by a Greek or Phœnician artiſt, in ſome part of the ſmall ſpace of time that the Parthians were maſters of Cilicia, than as proper Parthian coins. This appears to me, I own, extremely probable; but that it was an abſolute fact, I muſt not take upon me to affirm.

Thus have I endeavoured to render it probable, from a ſurprizing concurrence of circumſtances, and a wonderful coincidence of facts, that your Lordſhip's Phœnician coin was ſtruck at Tarſus, the capital of Cilicia, when the Parthians were maſters of that country, about forty years before the birth of CHRIST. Whether or no the credibility of ſuch a notion has been here fully evinced, your Lordſhip, whoſe profound erudition, as well as ſingular humanity, is ſo well known to the whole learned world, with your uſual candour and impartiality, will decide. Be this, however, as it will, I ſhall offer no other apology for any miſtakes or omiſſions that may be diſcovered in this paper, than that I have been obliged to tread unbeaten paths through the whole courſe of it; an explication of the medal in queſtion having, as I apprehend, been hitherto unattempted by any conſiderable writer. I might have expatiated more largely upon the preſent ſubject, but am afraid your Lordſhip will think me already too prolix.

prolix. I ſhall therefore at this time only beg leave to add, that by honouring me with your future commands, to which the greateſt and moſt ready attention ſhall ever be paid, you will confer the higheſt obligation upon,

My Lord,

Your Lordſhip's very faithful,

and moſt obedient humble Servant,

Chriſt-Church, Oxon.
April 25, 1767.

John Swinton.

ERRATUM, in *Philoſoph. Tranſact.* Vol. LIV. Page * 139. *note*, l. 2. for *שע read שצ*, or *Schin*, *Tzade*.

XXX. *Re-*

XXX. *Remarks on the very different Accounts that have been given of the Fecundity of Fishes, with fresh Observations on that Subject: By Mr.* Thomas Harmer; *communicated by* Samuel Clark, *Esq; F. R. S.*

Read May 28, 1767.

THE ascertaining the fecundity of the several species of fish, as far at least as we are able to do it, is one point necessary to the making our natural histories perfect; and at the same time opens a view wonderfully affecting to the imagination.

The carp, in which Petit is said to have found 342,144 eggs; and the cod, in one of which of middling size Lewenhoek, it seems, affirmed there were 9,384,000, have been mentioned as very surprizing instances of this fecundity; and by their being selected by writers, who appear to have been well versed in this part of learning, they should seem to be the most memorable we have of this kind.

The accounts, however, that have been given of the fruitfulness of these two species of fish differ from each other very considerably. For Bradley, the Botanic Professor at Cambridge some years ago, tells us in his philosophical account of the works of nature, a book professedly written on a very celebrated, though unexecuted plan,

plan *, that the increaſe in ſome fiſh is ſurprizing, and to thoſe that are not uſed to diſquiſitions of this kind muſt appear incredible; he however ſinks the number of eggs in theſe two ſpecies extremely, when he tells us the roe of a cod-fiſh muſt contain about a million of eggs, and that a carp does not ſpawn leſs than 20,000, to which he adds, and perhaps a tench half as many. This is making the cod almoſt ten times leſs prolific than the other account, and the carp above ſeventeen times leſs. Some other writers, who appear alſo to have been deſirous to impreſs the mind with the wonders of natural hiſtory, have made their eſtimate ſtill lower. One of them, I remember in particular, in one of our monthly publications, from whence numbers muſt take their ideas, who have no opportunity of reading more authentic accounts elſewhere, tells us, that carp and perch have nine or ten thouſand eggs, and that cod-fiſh, and herring, are not leſs prolific; and this he calls wonderful. The increaſe of cod-fiſh is indeed, even according to this, very great, but almoſt a thouſand times leſs than Lewenhoek is ſaid to have found it.

Their accounts being ſo very different, I thought I ſhould not improperly employ ſome leiſure hours, if I inquired into this matter afreſh, and ſaw what the fecundity of theſe ſpecies of fiſh really was, as well as of ſuch other ſorts as might fall in my way; and eſpecially as I had found that a ſmall pickerel, whoſe ſpawn I had taken a pretty exact account of, from mere curioſity, ſome time before, contained no fewer than 25,800 eggs: a fiſh which

* Propoſed by Mr. Addiſon in one of the Spectators.

none of these authors had mentioned, and of which notwithstanding a small one had produced a larger number of eggs than Bradley himself had assigned to the carp, which has been always looked upon as remarkable for its prolific quality, not to mention the unknown writer, who makes its fecundity much less.

The make of my eyes, which are much less proper for distinguishing objects at a distance, than the seeing small things that are near; and my living in a maritime country, and though not near the sea, yet in a situation which I thought very proper for the procuring such fish as I wanted, were additional motives to the search: though as to the last particular, I have since found myself greatly disappointed.

It will not be imagined, that, in order to ascertain the real number of eggs in each fish which I examined, I told them all over one by one; this would have been, if not absolutely impracticable, at least inconsistent with other engagements, and much more fatiguing than was necessary. My way was to weigh the whole spawn very exactly; then to take a piece weighing twenty, thirty, or forty, or more grains, as was most convenient, and after weighing that parcel with care, and giving the turn of the scales to the weights, not to the eggs, to tell them over very carefully; and then by dividing the number of eggs by the grains, to find how many eggs there were in each grain, or nearly so. I say nearly, for there must, according to this method, have been rather more; but I chose to estimate them after this manner, that there might be no danger of representing the fecundity of these animals greater than the truth

truth. I frequently boiled the portions of ſpawn that I told, and after macerating them ſome hours in water, gently preſſed them with a penknife, whoſe point I afterwards made uſe of to number them diſtinctly, by ſeparating them from each other to greater diſtances, after they were rendered by that gentle ſqueezing fitter for telling over.

In ſeveral fiſh I found their eggs of very different ſizes. In ſuch caſes my rule was, to tell all I could diſtinguiſh by my naked eye, and thoſe only; though I have often found, by the help of an eye-glaſs, that, by thus limiting myſelf, I paſſed over great multitudes of eggs that might juſtly have been counted*. I generally told them on a fine earthen

* For, though they were very different in ſize, they were all, I preſume, to be depoſited in the proper places for hatching that ſeaſon, though it may be not on the ſame day, ſince after fiſh are ſhotten, as it is called, we find no eggs at all in them. I think I do not by any means take upon me to affirm, that all fiſh depoſite their eggs after this manner, i. e. by degrees, and at times a little diſtant from each other; but ſome ſpecies of them, I ſhould think it is plain, do. This is the caſe of ſtickleback in particular, in which, when they have been extremely diſtended with ſpawn, I have found ſeveral eggs very large, but ſeveral hundreds very ſmall, and many of them too ſmall to be counted diſtinctly by an unaſſiſted eye, which ſmaller ones could never, I apprehend, grow to the ſize the larger ones had grown to before the larger ones were excluded. I have ſeen ſome of theſe eggs ſo large, that 24 or 25 would weigh a grain; about which ſize, I believe, they are excluded, ſince ſome of that bulk came away from one of thoſe creatures after it was taken out of the water, and were found in the paper in which it was wrapped up: but generally upon opening theſe fiſh they are not quite ſo large, though very large in compariſon of many of the reſt. I told in one of theſe ſmalleſt of fiſhes, which weighed very little more than 14 gr. ſpawn and all, about 936 eggs, ſome of which almoſt eluded my eye, beſides

 veſſel,

veſſel, which was extremely black, by which means I could much better diſcern them, than I otherwiſe ſhould have been able to do. The weights I have reckoned by are Avoirdupois weights; but there being no weights of that ſort ſmall enough to anſwer all purpoſes, I was obliged to make uſe of grains along with them, of which I reckon $437\frac{1}{2}$ make an ounce Avoirdupois. After this manner I made the obſervations of which I am going to give an account, with all the nicety and care I was capable of.

I begin with the herring, which makes a diſtinguiſhed figure in theſe two counties of Norfolk and Suffolk, and a conſiderable part of our commerce, when ſalted and ſmoked. One of the above-mentioned authors ſuppoſed, they may have 9 or 10,000 eggs: of ſeveral I examined, I found none which had ſo few as 20,000; and in one I found 36,960. The intermediate numbers which appeared to me on

numbers of others that I could not tell at all. Thoſe that I told were of all ſizes; and though there were but 56 very large eggs, yet the creature was extremely diſtended. Now it does not appear poſſible that all the 936 eggs ſhould grow to the bulk of the largeſt of their companions without deſtroying the fiſh, ſince the growing of 56 to ſuch a ſize as to weigh about $1\frac{1}{4}$ grain diſtended it in ſuch a manner as I have hardly ſeen in any other ſpecies of fiſh. In ſome very large ſticklebacks, which weighed 45 or 50 grains, I have found between 2 and 300 of theſe large eggs, along with great numbers of ſmaller ones, ſo that the number of large eggs ſeems to bear ſome proportion to the ſize of the fiſh, by which means a greater number of eggs, anſwerable to their ſize, may be depoſited in the ſame time in which the ſmaller fiſh of this ſpecies diſcharge their leſs numerous ſpawn. I will only farther obſerve here, that if this be the caſe as to the other fiſh, the number of their eggs may, on this account, appear very different to different obſervers.

examination,

examination, may be met with in a table that I ſhall place at the end of this paper, which will give the particulars relating to this ſort of fiſh, and ſeveral others, in the ſhorteſt manner, but with ſufficient diſtinctneſs.

The next that came under my inſpection was the ſmelt. Theſe, it is well known, are a very ſmall ſort of fiſh, and are frequently uſed for garniſh to thoſe that are larger. In one of theſe, which did not weigh quite two ounces, I found 38,272 eggs; and in none ſo few as 20,000; excepting one, which was extremely ſmall, not weighing above 289½ grains, in which very ſmall fiſh I found 14,411. This was amazing.

I was much more ſurprized, when, after this, I learnt what was the fecundity of mackarel. This no author that I met with gave an account of, though it is a fiſh ſo extremely common. In one large fiſh of this kind, weighing ſomewhat better than 1¼ ℔, I found 454,961 eggs; in a ſecond, of much the ſame weight, 430,846; and in a third, which weighed but about 1 ℔ 2 oz. I found 546,681.

I was aſtoniſhed upon this, that Bradley ſhould call the ſuppoſed fecundity of carp, which he makes to be but about 20,000, ſo ſurprizing; or that even Petit's obſervation, which made it appear, that in ſome fiſh of that ſpecies the eggs amount to 342,144, ſhould cauſe the carp to be ſelected as the moſt extraordinary fiſh for increaſe, after the cod, when it appears to be ſo much greater in mackarel (which is at the ſame time ſo common a fiſh), as to be not much ſhort of the proportion of 5 to 3, in the laſt I examined.

I ſhall

I ſhall leave it to the table at the cloſe, to give an account of the prolific quality of ſome other fiſh, whoſe eggs I have counted, and ſhall paſs on to what I have found in the carp, which ſpecies Petit examined. As to this fiſh, though I cannot ſay I have found the eggs, in thoſe that have come under my notice, ſo numerous as he did; yet, as I have found the number much larger than Bradley mentions, ſo I make no doubt but that Petit really found them amount to 342,144; and I would add, that I dare ſay they may be found to be much more numerous ſtill in large fiſh of this kind, ſince in one that I examined, weighing but 16¾ oz. I found 101,200; and in another, which weighed no more than 25½ oz. I found 203,109, and carp grow to a much larger ſize than the biggeſt of theſe, but I could not procure any of thoſe large carp that were full of ſpawn.

If I failed in verifying Petit's number of eggs in a carp, I found thoſe of a tench to exceed it, more than once, which Bradley reckons not to produce more than half the number a carp does, or 10,000. For I found in one tench, which weighed 2¼ lb, 383,252 eggs at leaſt; and in another that weighed not quite 1¾ lb, 350,482.

As to perch, which one of thoſe authors I mentioned puts upon a par with carp, I could get none of any ſize. The largeſt, which weighed but 8 oz. 9 dr. contained 28,323 eggs; and a ſecond of 5 oz. 10 dr. had in it 20,582. They ſeem, however, to be much leſs prolific, in proportion to their bulk, than tench, ſince the largeſt of theſe had but about 28,323 eggs, and I found a tench of nearly that ſize, weighing but 8 oz. 14½ dr. produced 83,104.

Bradley

Bradley ſeems to have been as careful not to be guilty of exaggeration with reſpect to the cod-fiſh, as in the other caſes. He eſtimates them at a million, while Lewenhoek affirmed that he found above nine millions of eggs, in one of middling ſize. The ſpawn of one of that ſize which I examined, that is to ſay, one of 18 or 20℔ weight, I found to contain between three and four millions, if my friend's weights were accurate, for I made the examination at a diſtance from home; and by a ſubſequent obſervation at home, I have reaſon to think there was this number in it, though I was prevented purſuing my ſecond examination ſo far as I would have done, by ſome particular occurrences. According to this, there is nothing incredible in Lewenhoeck's account; his fiſh, I ſuppoſe, being remarkably diſtended with ſpawn, and for that reaſon perhaps thought, by that inquiſitive and curious perſon, a ſubject that ought not to paſs unexamined.

I have hitherto mentioned no flat fiſh, nor do I remember that any author has given us an account of their fecundity. I imagined, from their make, it could not be extraordinary; I was therefore extremely ſurprized to find in the firſt flounder I examined and which did not weigh quite three ounces, 133,407 eggs; in a ſecond, which weighed little more than $3\frac{1}{2}$ ounces, 225,568; and much more ſtill when I diſcovered in a large one, that weighed about $24\frac{1}{4}$ oz, and which was of that ſort, that is ſpotted like a plaiſe, as Ray has told us ſome flounders are, 1,357,400: this was truly aſtoniſhing.

The number of eggs that a ſoal produces, I have obſerved to be great, but nothing like that which I

6 found

found in flounders, finding in one, which did not fall much short of a pound, rather more than 100,000; and in another, that weighed about five ounces, 38,772.

To make this disquisition still more extensive, I examined two or three kinds of shell-fish. I found in a lobster of $14\frac{1}{2}$ oz. when in the shell, and of $10\frac{3}{4}$, when divested of it, 7,227; and in another that weighed $2\frac{1}{4}$ lb, and out of the shell somewhat better than $1\frac{3}{4}$ lb, I told 21,699 eggs.

I took also the pains to tell all the eggs of some shrimps, one by one, and found in one which weighed $17\frac{1}{2}$ grains only, 3057; in another of 31 gr. 4090; and in a third of 39 gr. 6807.

This, considering the smallness of the creature, is more remarkable than the fruitfulness of the lobster; but neither is the one or the other to be compared, in this respect, to the crab, for in a large one, weighing near $1\frac{1}{2}$ lb, but not quite, I found that the spawn weighed 687 gr. but the eggs were so minute, and at the same time adhered so close together, that I could not number them distinctly; however, the weight of the whole, and the minuteness of the eggs, show that they must have been very numerous; and I believe from a rough estimate I made, that they exceeded a million.

The table, at the close, gives all these accounts, with the addition of several more particulars, in a very short compass. The first column contains the names of the fish which I examined; the second their weight; the third the weight of their spawn; the fourth their fecundity; and, as I supposed some persons might be desirous to know how large a

portion

portion of ſpawn I weighed in each caſe, I have ſet down the number of grains in each ſuch portion, in a fifth; the number of eggs found in a grain, by this method is ſeen in a ſixth, by which we may give ſome gueſs at the different ſizes the eggs of each ſpecies are of, when they are excluded; and I have made the time of examining each fiſh reſpectively a ſeventh; which poſſibly may be of ſome uſe to thoſe who may have an inclination to repeat any of theſe obſervations, as from thence may be learnt ſomething concerning the moſt advantageous time of examining theſe creatures, which certainly ought to be as nearly as we can, when the eggs are come to their full ſize, and before any of them are depoſited. However, after all, if my notion is juſt, that ſome ſpecies depoſit a part of their eggs come to their full growth, before others laid the ſame year are big enough to be told with diſtinctneſs, the accounts of the fecundity of ſuch fiſh muſt be extremely defective; and this I apprehend, amongſt thoſe I have examined, is the caſe of mackarel, carp, tench, and ſome others; in herring, &c. there does not appear ſuch a difference in the ſize of different eggs.

From this table it appears, that the ſize of the eggs is nearly the ſame in great and ſmall fiſhes of the ſame ſpecies, at the ſame time of the year; that the quantity of ſpawn is, uſually, nearly proportionate to the ſize of the animal, from whence we may give a tolerable gueſs at the greateſt fecundity of each ſpecies, if we know to what weight they have been found to grow while in a breeding ſtate; we may likewiſe ſettle their produce at a medium, upon learning what is the mean ſize of each ſpecies

when

when in the forementioned ſtate, but we ſee, however, that this is not univerſal, nor conſequently perfectly exact, ſome fiſh being much more prolific than others of the ſame ſize, and ſpecies.

To conclude, the great fecundity of fiſh is not the only thing that affects the imagination, when we are examining matters of this ſort: the extreme diſproportion between their ſize when they firſt appear in the water after hatching, and that of their full-grown ſtate, as well as the little proportion that is to be obſerved between the bulk of fiſh of different ſpecies and that of their eggs, are things that are very amazing to perſons of a curious turn. The egg of a ſmelt, which at its full growth weighs but two or three ounces, appeared, in thoſe I examined, to be larger than thoſe of a cod-fiſh, which weighed eighteen or twenty pounds, and might have grown to double that bulk; and that of a ſtickleback, which is the ſmalleſt of all known fiſh, was found to be above ſix times bigger than the largeſt I ever obſerved in a ſmelt. What becomes of ſuch amazing numbers of young fiſh, and why ſome are made ſo extremely prolific, the flounder and crab in particular among the ſmaller ſorts, would doubtleſs be highly entertaining ſubjects, if duly illuſtrated; but theſe are enquiries I have no opportunity of making.

The TABLE.

1. Names of the fish.		2. Their weight	3. Weight of the spawn	4. Fecundity	5. The portion of sp. weighed	6. N°. of eggs to a grain	7. Time of exam.
		oz. dr.	grains.		grains.		
Carp	N°. 1	16 12	1265	101.200	46	80	May 25
	N°. 2	25 8	2571	203.109	55	79	April 4
Cod-fish		———	12.540	3.686.760	29	294	Dec. 23
Flounder	N°. 1	2 14	182½	133.407	23	731	Feb. 21
	N°. 2	3 8½	152	225.568	19	1484	Dec. 18
	N°. 3	6 12	598	351.026	26¾	587	March 14
	N°. 4	24 4	2.200	1.357.400	24½	617	d°.
Herring	N°. 1	4 3	367	32.663	48	89	Oct. 8, 1763
	N°. 2	5	236½	21.285	48½	90	29
	N°. 3	3 13	259	23.569	52½	91	Oct. 2, 1764
	N°. 4	5 10	480	36.960	53	77	25
	N°. 5	4 6½	366	29.646	57	81	d°.
	N°. 6	4 8	420½	27.753	51	66	Nov. 3
	N°. 7	5 1	490½	32.863	41½	67	Oct. 18
Lobster	N°. 1	14 8	———	7227	———	14	April 4
	N°. 2	36 0	1671	21.699	129	———	Aug. 11
Mackarel	N°. 1	20 —	1027	454.961	33	443	June 20, 1764
	N°. 2	20 —	949	430.846	24½	454	29
	N°. 3	18 —	1223½	546.681	32½	447	18, 1765
Perch	N°. 1	8 9	765½	28.323	85	37	April 5
	N°. 2	5 10	502	20.582	85	41	6
Pickerel	N°. 1	56 4	5100½	49.304	70	9⅔	April 25
	N°. 2	———	3248	80.388	76½	24⅔	Nov. 25
	N°. 3	48 10½	3184	33.432	43	10½	March 19
Prawn	N°. 1	(127 gr.)	———	3.806	———	243	May 12
	N°. 2	(94½ gr.)	———	3.479	———	287	d°.
	N°. 3	(100½ gr.)	———	3.579	———	247	d°.

Roach

1. Names of the fish.		2. Their weight		3. Weight of the spawn	4. Fecundity	5. The portion of sp. weighed	6. N°. of eggs to a grain	7. Time of exam.
		oz.	dr.	grains.		grains.		
Roach (or what I took to be of that species)	N°. 1	2	—	114	9.604	—	—	April 4
	N°. 2	6	8	671	43.615	68	65	May 4 1764
	N°. 3	3	8	346½	29.799	42½	86	d°.
	N°. 4	2	2	153	9.486	42½	62	5
	N°. 5	10	6½	361	81.586	39	226	2 1765
	N°. 6	9	10¾	417	113.841	42	273	6
	N°. 7	3	8	213½	45.475	20	213	24
Shrimp (with light coloured spawn.)	N°. 1	(17½ gr.)		3	3.057		1000	May 3
	N°. 2	(39 gr.)		7	6.807		972	d°.
	N°. 3			—	4.601		—	d°.
Ditto (with dark colour)	N°. 1	(31 gr.)		5	4.090		818	d°.
	N°. 2	(22 gr.)		4	2.849		712	d°.
Smelt	N°. 1	2	—	149½	38.278	30	256	Feb. 21
	N°. 2	(289½ gr.)		50	14.411	—	288	Mar. 21, 1764
	N°. 3	1	14	157½	29.925	40½	190	27, 1765
	N°. 4	1	12	145½	30.991	20	213	28
	N°. 5	1	7	149	24.287	20	163	d°.
	N°. 6	1	5	136	23.800	20	175	d°.
Soal	N°. 1	14	8	542½	100.362	20	185	June 13
	N°. 2	5	—	179½	38.772	20	216	28
Tench	N°. 1	40	—	—	383.252*	—	—	May 28, 1764
	N°. 2	28	8	533½	280.087	25	525	3, 1765
	N°. 3	8	14¾	224	83.104	20	371	10
	N°. 4	9	8	284½	108.963	20	383	d°.
	N°. 5	12	8	366	138.348	22½	378	d°.
	N°. 6	27	9½	1969	350.482	23	178	June 11
	N°. 7	14	15	866	138.560	20	160	d°.

* N°. 1. of the tench certainly had a much larger number of eggs; but being extremely distended with spawn, and unluckily let fall before it was brought to me, the enveloping skin in which the eggs were contained was broke, which made it difficult to determine some circumstances relating to this fish; it however had the number of eggs I have set down, at the lowest way of reckoning, and I believe many thousands more.

I have taken no notice of several fractions in the number of eggs contained in a grain in many cases, choosing to fall rather below than to exceed the truth, in all the fish I have given an account of in this table. I have been scrupulously exact in all particulars, excepting what are contained in the second column, which gives the weight of the fish I examined, in which the utmost nicety was not necessary: some few might weigh a little more or a little less; but all were nearly of the weight set down, and much the greatest part exactly so.

XXXI. An

XXXI. *An Account of an Hydro-enterocele, appearing like an Hydro-ſarcocele, and ending in the Death of the Patient, in which the Inteſtine had paſſed from the Hernial Sac, into that of the Hydrocele by which the Strangulation was formed. Communicated by Mr.* Le Cat, *F. R. S. &c. in a Letter to* Charles Morton, *Sec. R. S. Tranſlated by* J. O. Juſtamond, *Surgeon.*

Read May 28, 1767.

IN the year 1751, I preſented to the Royal Society an obſervation of a rupture, which had two hernial ſacs; at preſent, I have the honour to ſend them the hiſtory of another kind of rupture with a double ſac, much more fallacious and dangerous than the former, ſince I found myſelf deceived by it, after the experience of forty years.

James Philip Chiquet, aged 65 years, of the Pariſh of St. Vivien at Rouen, was admitted into our hoſpital on the 24th of January 1767.

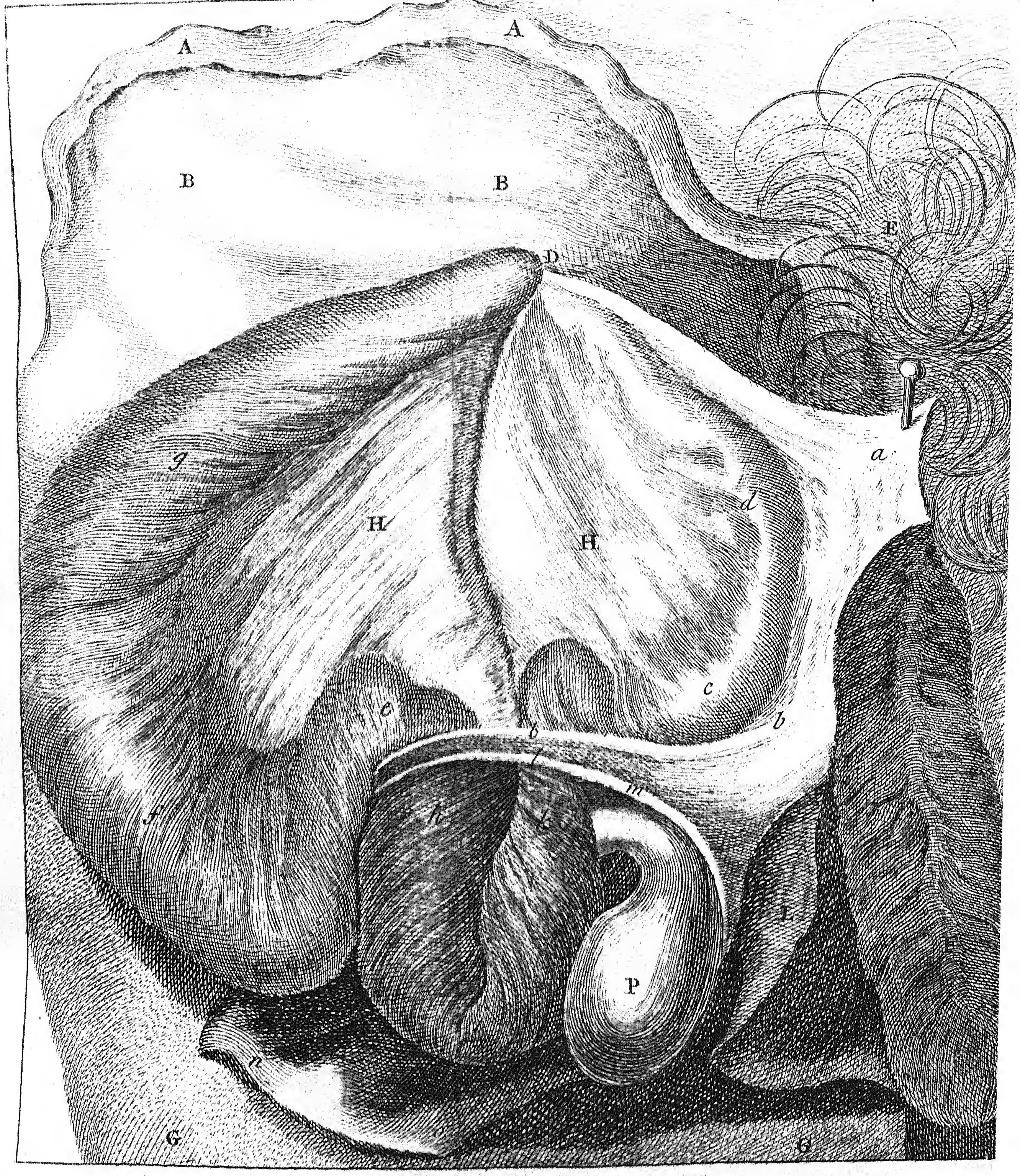
The account, which he gave of himſelf, was, that he had been accuſtomed to a rupture, which he had not been able to reduce for a fortnight paſt, and, that ſince eight days he had been ſeized with a vomiting, and was incapable of taking any nouriſhment.

ment. Upon examination, the tumor was ſoft, eſpecially at the upper part towards the ring, which ſeemed to be ſo free and diſengaged, that the finger with the integuments might be puſhed under it: the large cord, which came down to it, was flat, ſoft, and appeared to be compoſed entirely of the ſpermatic veſſels enlarged.

The extremity of the ſwelling, which was of the ſize of a large orange, was evidently a very tranſparent hydrocele; at the baſis of which ſome hard points were to be felt, which I thought to be ſchirrous tumors. I concluded, therefore, that his complaint was an old hernia, ſucceeded by a ſarcocele and an hydrocele, and that the inteſtine was at that time returned. I imagined that the vomitings, which were not frequent, might be cauſed by ſome other diſorder, perhaps by the progreſs which the ſarcocele might have made in the cavity of the abdomen; and I alſo thought that the weak and almoſt dying ſtate he appeared to be in, was a prognoſtic of the fatal manner in which thoſe caſes uſually terminate; for his ſtrength was ſo far exhauſted, that he expired in the following night.

I was very deſirous of examining the caſe, having been always induced to ſuſpect, from the vomitings, and the flatneſs, ſoftneſs, and ſize of what paſſed through the ring, that there was a deſcent of the inteſtine.

See Tab. XIII. Upon opening the common hernial ſac, *a. b. b.* a large portion of inteſtine, *c. d. e. f. g.* preſented itſelf, which was very flaccid and almoſt empty; but what ſurpriſed me moſt, was, to find that the convoluted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voluted extremity, *b. i. k.* of this inteſtine, had inſinuated itſelf into the ſac, *l. m. n. o.* of the hydrocele, which was formed by the vaginal coat of the teſticle, P. and, that only this portion of inteſtine, *b. i. k.* was ſtrangulated, hard, and changed in colour. The redneſs was ſo ſlight, that this ſtrangulation could ſcarcely have been the immediate cauſe of his death; but I rather imagined that an univerſal decay, and waſte of ſtrength, for a long time paſt, had contributed to haſten this event.

If the patient had lived, my deſign was, firſt, to have drawn off the water of the hydrocele by an opening made large enough to have examined into the contents of the tumor, which were but indiſtinctly felt, on account of the ſurrounding water.

According to the diſcovery I ſhould have made by this opening, I ſhould then have purſued my operation, whether I had met with a ſarcocele or a rupture. In order to put theſe intentions into execution, I had previouſly ordered the hair to be ſhaved off the tumor, and, on account of his great weakneſs, forbad bleeding, which had been ordered at firſt. This operation would probably have ſaved his life, if he had come into the hoſpital a few days ſooner, and his ſtrength had been ſufficient to carry him through it.

Explanation of Plate XIII.

A. The integuments of the abdomen raiſed. B. The external oblique muſcle. D. The ring. E. The Pubis. F. The Penis. G. The right thigh. H. The meſentery. L. Part of the dartos.

a. b. b. Part of the hernial ſac.

c. d. e. f. g. Portion of inteſtine contained in this ſac.

h. i. k. The convoluted extremity of this inteſtine contained in the ſac of the hydrocele:

l. m. n. o. The ſac of the hydrocele compoſed of the vaginal coat of the teſticle.

P. The teſticle.

XXXII. *Novorum quorundam in re electrica experimentorum Specimen, quod Regiæ Londinensi Societati mittebat die 26 Aprilis* 1766, Joannes Baptista Beccaria, *ex Scholis Piis, R. S. Soc. communicated by* M. Maty, *Sec. R. S.*

Experimentum primum.

Read June 4, 1767.

EXPERIMENTUM captum Pekini a PP. Soc. JESU, & anno 1755, missum academiæ Petropolitanæ, [a] analysi persequi studens, laminam vitream lævem A charta inaurata rite indutam faciebam electricam, immisso in ipsam igne a catena; tum exutam imam ipsius faciem admovebam faciei superiori laminæ similis B utrinque nudæ, nec ullatenus electricæ, quæ facie sua ima circello imminebat chartaceo sesquipollicari [b].

II. Porro, nisi a facie superiore vitri A ignem redundantem haurirem aliquomodo, circellus non commovebatur; ubi attingebam ejus faciei indusium, circellus emicabat ex abaco, oscillabatur;

[a] Vide Nov. commentar. Acad. Scien. Imp. Petrop. tom. viii. pag. 276.

[b] Lamina A pendebat uncias 6. octavas unciæ partes 7; lamina B pendebat uncias 6. octavas unciæ partes 4. denarium 1; utriusque laminæ latitudo erat pollicum 6. linearum 10; longitudo pollicum 8. linearum 11; latitudo indusiorum pollicum 4; longitudo pollicum 5.

tur; imo erectus circuibat quasi saliens sub ea lamina; quales aptissimas positiones, & motus facit sibi in traducentibus corporibus trajiciens electricus ignis. Ii motus, dum divellebam laminam A, abrumpebantur; ea iterum admota, reviviscebant motus eorum omnium residui, quos facere debuisset attrectatio indusii laminæ A, nisi fuisset ipsa dimota; attrectatio alia similem, sed debiliorem motuum, seriem excitabat, atque ita concidebat vis explodendi, & quatiendi in lamina A.

III. Quum primo disjungerem laminam A a lamina B, jam percipiebam, insolita vi aliqua cohærere mutuo ambas; eamque cohæsionem experiebar majorem, prout tardius disjungebam laminam A, post plures scilicet attrectationes. Si, cum primo laminam A imposueram laminæ B, induebam extimam hujus faciem, attrectando simul indusia junctorum vitrorum, quatiebar: valida repente existebat ab ea explosione vitrorum cohæsio, eademque validior pro explosione vehementiore.

IV. Quum laminas ante explosionem jam aliquomodo cohærentes disjungerem, lamina A utraque facie apparebat electrica excessu, & lamina B utraque facie apparebat electrica defectu; quum laminas easdem disjungerem post explosionem, electricitates apparebant contrariæ; scilicet lamina A apparebat utrinque electrica defectu, lamina B utrinque excessu. *Quam mutationem primam nomino ex explosione electricitatum oscillationem.*

V. Si post explosionem alternatim disjungebam, & conjungebam laminas prope circellum chartaceum (I.), circellus in quaque disjunctione adhærebat laminæ

laminæ B, in quaque conjunctione discedebat, idque vel quingenties; quod est experimentum Pekinense, quodque innuit *alias, quæ fiunt ex disjunctione et conjunctione vitrorum, electricitatum oscillationes.*

VI. Has scintillis facile imitabar: vitrum A electricum a catena (I.) exutum ima facie imponebam nudæ faciei vitri B; indutam faciem extimam hujus vitri, & indusium vitri A una attrectabam; quatiebar: explosionem consummabam iterata attrectatione, aut aliter; tum vitra juncta juxta latus unum meo apprimebam stomacho summis ipsa prehendens juxta latus oppositum manus utriusque digitis, quo disjungere alternatim ipsa possem, atque iterum conjungere. Porro manus habebam ita dispositas, ut auriculari digito manus lævæ indusium laminæ B possem attingere, & pollice manus dexteræ possem similiter attingere indusium laminæ A. Hæc tunc observabam.

VII. 1. Si ante, & post disjunctionem vitrorum indusia eorum ambo attrectarem, scintillas percipiebam utrinque; sed alacriores, quæ existebant a disjunctione: at istæ erant ferme unicæ. Quæ autem conjunctionem sequebantur, erant multiplices, ut viderentur multiplicitate sua illarum vim exæquare; atque si eadem lege pergebam vel octingenties, uti sum semel expertus, vitra divellere, atque conjungere, scintillæ nondum omnino peribant. 2. Si disjunctis vitris, neutrum attrectabam, post eorundem conjunctionem scintillas nullas percipiebam, percipere poteram in sequente disjunctione; similiter si conjunctis vitris, neutrum attrectarem, post disjunctionem scintillas non percipiebam,

cipiebam, percipiebam post sequentem conjunctionem. 3. Si post disjunctionem attrectarem vitrorum unum dumtaxat, aut nullæ, aut omnino exiguæ post conjunctionem scintillæ percipiebantur; similiter, si post conjunctionem attingebam unicum vitrum, scintillæ post disjunctionem aut sentiebantur nullæ, aut vix sentiebantur aliquæ.

VIII. Re ita scintillis expensa, penicillo, & stellula earum scintillarum directionem persequebar: facies superior vitri A, quod unum evaserat electricum a catena, hisce in divulsionibus post explosionem penicillum, facies extima vitri B stellulam efformabat in objectis apicibus; & exploratore immisso inter vitra divulsa (filo nempe ferreo abeunte in ramos duos) facies intima vitri A penicillum, intima vitri B stellulam efformabat; scilicet vitrum A a divulsione post explosionem erat utrinque electricum defectu, vitrum B utrinque excessu.

IX. Contra in conjunctione facies extima vitri A efformabat stellulam, facies extima vitri B in exhibitis apicibus penicillum faciebat; quæ duæ res (VIII. IX.) veram demonstrant, continuatamque electricitatum ex disjunctione, & conjunctione oscillationem.

X. Ipso conjunctionis, & disjunctionis momento facies internæ quid facerent, nequivi cernere; sed ex hac experimentorum serie poterit quisque arguere.

XI. Atque hæc quidem altera ex vitrorum conjunctione, & disjunctione oscillatio jucunda est admodum; sed & aliam præterea licet cum ipsa componere, quæ efficitur ex inversione vitri præpol-

 lentis;

lentis; in hoc enim experimento vitrum illud, quod principio factum est electricum a catena, quum facie contraria tangit vitrum aliud, contrarias & in ipso se, & in vitro socio electricitates facit, tum quæ sunt ex disjunctione, tum quæ conjunctionem sequuntur; seu planius: si inter hæc experimenta invertitur vitrum A, ut facies, quæ erat extima, denudata evadat intima, & facies, quæ erat intima, fiat extima, & induatur, nova aliqua post aliquod tempus ex apta attrectatione oppositarum facierum insurgit vitrorum cohæsio; & vitrum A, quod a disjunctione apparebat utrinque electricum defectu, nunc apparet utrinque electricum excessu; vitrum B, quod a disjunctione apparebat utrinque electricum excessu, nunc apparet utrinque electricum defectu; & in conjunctione vitrum A, quod apparebat electricum excessu, nunc apparet electricum defectu; vitrum B, quod in conjunctione apparebat electricum defectu, nunc apparet electricum excessu. Atque tanta est præpollentis vitri A vis, ut, sive interea vitrum B non invertatur, sive ipsum etiam invertatur, descripta electricitatum mutatio semper contingat, nulla unquam contingat, si solum vitrum B invertatur. Præterea si vitrum A restituatur suo loco, ut facies ipsius, quæ principio fuerat intima, & ab inversione evaserat extima, iterum denudetur, & fiat intima, iterum redeunt eædem, quæ principio fuerant electricitates, iterum mutandæ ab alia inversione vitri A, iterum restituendæ ab ejus vitri restitutione in positionem primam. Atque ita deinceps.

XI. Porro

XII. Porro vitrorum inverſiones contrarias electricitates conſequentes ex vitrorum conjunctione, et disjunctione, debilitant longe magis, quam debilitent ſolæ disjunctiones, conjunctioneſque nulla interpoſita vitrorum inverſione; potui tamen cœlo mediocriter ſicco quinquies & decies memoratas electricitatum ex inverſione vitri A mutationes obſervare, & interponere præterea inverſiones plures vitri B, quo viderem, his electricitates non mutari.

XIII. Quas tamen obſervationum progreſſiones ut aſſequi poſſimus, primum, ut innuebam, tempeſtas facit, tum vero mutationibus electricitatum pervidendis opportuna vitrorum junctorum poſt inverſionem mora, & attrectatio eſt plane neceſſaria; mora autem ſufficiet brevior, prout vitrorum junctorum facies extimæ corporibus attrectabuntur ad ignem electricum hauriendum, immittendumque aptioribus. Ego quandoque, cum citius vitra disjungerem, aut electricitates nondum mutatas in contrarias, aut contrarias nondum ſatis invaluiſſe, videbam, & faciebam, ut invaleſcerent, vitra iterum jungendo faciebus iiſdem, apteque attrectando.

XIV. Ad reliqua quod attinet experimenti adjuncta ea ſunt pleraque omnino obvia. Unum memorabo, quod & ipſum, quamquam ſe prodat ſatis aperte, meretur tamen recenſeri ideo, quod ſingularem aliquam habeat ſignificandi vim. Itaque cum primo ab opportuna poſt exploſionem mora vitra disjunguntur, tum ſcintillæ exiſtunt inter angulos induſiorum, & digitos, quibus nudos vitrorum angulos prehendo, omnino vividæ; intereaque ſcintillarum crepitus exauditur undique

plurimus,

plurimus, atque, si res agatur in tenebris, lux plurima circa margines indusiorum, & plurima emicat intra intimas vitrorum facies: tanta nempe est vis, qua a disjunctione turbatur æquilibrium, quod jam ferme obtinuerat ante disjunctionem. Neque hæc phænomena in ea prima disjunctione finiuntur, sed, quamquam usque & usque debiliora, apparere tamen pergunt in disjunctionibus aliis, & si cætera sint paria, alacriora obveniunt, prout rapidiore motu distrahuntur vitra, & prout a debita mora, & attrectatione invaluerunt magis novæ pro nova disjunctione electricitates. Sive autem experirer faciebus vitrorum indutis, sive nudatis faciebus vitra conjungerem, atque disjungerem, phænomena semper existebant homologa.

Experimentum secundum.

XV. Hæc experimenti Pekinensis analysis deducebat me ad experimentum Simmerianum explorandum & promovendum. Utebar iisdem duobus vitris A & B, quæ juncta faciebus nudis induebam faciebus duabus aliis extimis, & aptabam catenæ, ut fierent electrica [c]. Intereaque jucundum erat mihi oculis cernere invalescentem in ipsis electricitatem; etenim circa indusia zonæ coloratæ Newtonianæ conformabantur, quas commode cernebam in distantia pedum quatuor, & distinguebam numero sex. Eæ parallelæ erant omnes inter se, & erant pariter ferme parallelæ indusiorum limitibus. Ferme, inquam, nam circa indu-

[c] Vitra semper adhibui prot e tersa & sicca ab igne.

ſiorum angulos, quos, ne electricitatem perderent, rotundaveram, cæ zonæ arcuabantur in ſemicirculos ab induſiis extrorſum prominentes magis, quam zonæ parallelæ lateribus; quamquam hæ etiam ſinuabantur introrſum ubi induſiorum latera vitris non adhærebant. Sed de his alias plura, nec, ut arbitror, inutilia; ſunt enim hæ zonæ veſtigium conſpicuum electricitatis ſe effundentis circa limites rectilineos, arcuatos, acutos, &c.

XVI. Vitra quum cernebam omnino electrica, dimovebam a catena, attrectabam in extimis induſiis, fiebat exploſio, quatiebar, neque interea ab exploſione mutari animadvertebam coloratas zonas indicia cohæſionis.

XVII. Re enim vera obniti debebam valide admodum, ut vitra memorata disjungerem; quam primum vero hanc disjunctionem me aſſequi ſentiebam, evaneſcebant zonæ coloratæ; intereaque ſcintillæ, crepitus, lux, & phænomena exiſtebant omnia, quæ memoravi (XIV), imo iis alacriora.

XVIII. Porro ſi vitra disjungerem anteaquam attrectando extima induſia exploſionem cierem, vitrum ſuperius apparebat utrinque electricum exceſſu, inferius utrinque defectu: Si deinde exploſionem conficerem, a disjunctione, quæ poſtea fiebat, mutatæ apparebant electricitates; vitrum ſuperius erat utrinque electricum defectu, inferius exceſſu.

XIX. Si vitra poſt exploſionem disjungere, & conjungere pergebam, quemadmodum in ſuperiore experimento, eædem, quæ in illo, imo vividiores conſequebantur electricitatum oſcillationes, quas ſimiliter penicillo expendebam, & ſtellula.

XX. Neque

XX. Neque hic deerat vitrum præpollens, quod aliam componeret ex ſua inverſione cum ea oſcillatione electricitatum oſcillationem. Hæc una erat, ipſaque aptiſſima differentia: Vitrum præpollens in ſuperiore experimento illud erat, quod unum fiebat electricum a catena. At in hoc experimento, in quo ambo junctim fiebant electrica, præpollebat vitrum tenuius [d], ſive ipſum catenam tangeret, dum fiebant electrica a catena, ſive eſſet inferius, & communicaret cum ſolo.

XXI. Præpollens vero vitrum hoc ſuam vim primo demonſtrabat in exploſione, quæ tentaretur in duobus vitris ſeorſim: ſcilicet ſi poſt exploſionem junctorum vitrorum intimis faciebus disjunctorum aptabam induſia, vitrum præpollens me quatiebat mediocriter, aliud non item.

XXII. Itaque ſi, quemadmodum in ſuperiore experimento, ita hic poſt exploſionem invertebatur vitrum pæpollens, electricitates a disjunctione, et conjunctione exiſtentes mutabantur ſimiliter in contrarias, reſtituendæ reſtituto vitro præpollente, iterum mutandæ eo iterum inverſo, neque mutandæ inverſione vitri alterius ſola.

XXIII. Atque hanc hujus ſuæ vis tenacitatem pergebam adhuc aliter experiri in vitro præpollente. Duo vitra junctim electrica a catena ſtatim divellebam, qua prima in disjunctione ſcintillæ, lux, crepitus, phænomena obtingebant omnia iis ſi-

[d] Plura in his experimentis adhibui vitrorum paria, & ſemper tum in hoc, tum in experimentis, quæ ſequuntur, illud præpollebat, quod erat tenuius; nunc in par vitrorum incidi, quorum alterum, quod præpollet, craſſius videtur, ſed videtur etiam denſitate minore.

 milia,

milia, quæ memoravi (XIV); tum induebam intimas eorum facies, vitrum præpollens attrectabam, quatiebar; consummata explosione ipsum aptissime attrectabam (XIII), post minutum temporis aptabam vitro socio, quod interea pependerat in aëre summo angulo suspensum; juncta attrectabam, vix existebat explosio; disjuncta electricitates monstrabant iis contrarias, quas solent ostendere ante explosionem. Atque tum a disjunctione, & conjunctione consuetæ post explosionem electricitates oscillabantur; istæque invertebantur inverso vitro præpollente.

XXIV. Vitra eadem junctim electrica tentabam, ut exploderent juncta, deinde sejuncta, iterum juncta, iterum sejuncta; postea attrectabam dudum seorsim singula (XIII) ad minutum temporis, iterum juncta eadem, qua principio fuerant, positione, post debitam moram, & attrectationem, electricitates demonstrabant singula contrarias electricitatibus ante explosionem; & tum etiam ab inversione vitri præpollentis eæ electricitates abibant in contrarias.

Experimentum tertium.

XXV. Duo vitra A & B singula rite induta singulis catenæ ramis objiciebam, ut fierent seorsim electrica; atque, ut æquilibratas electricitates servarent, quales nempe eodem tempore a catena eadem immitti potuerant, ea cautione a catena ipsa dimovebam, ut neuter ejus ramus cum solo communicaret nisi post semota vitra ambo.

XVI. Mox

XVI. Mox denudatis duabus imis vitrorum faciebus, (quod dum fiebat, ſcintillulæ exiſtebant ſingulæ ex ſingulis imis vitrorum faciebus) aptabam vitrum alterum alteri, ut faciebus denudatis ſe mutuo contingerent; atque tum nec ex attrectatione extimorum induſiorum ulla exiſtebat exploſio, nec ullam percipiebam cohæſionem in disjunctione: ſejuncta, atque iterum induta quatiebant valide admodum, ſed tenuius validius; iterum juncta faciebus iiſdem denudatis dudum attrectabam, cohæſio obtinebat aliqua; disjungebam, & tenuius, quod præpollebat in ſuperiore experimento, in hoc item erat præpollens; ipſum ab hac disjunctione electricum defectu apparebat utrinque, & vitrum ſocium apparebat utrinque electricum exceſſu, inverſo vitro tenuiore, electricitates a conjunctione, & disjunctione ibant in contrarias, inverſo ſocio craſſiore manebant eædem.

XXVII. Poſt hæc capiebam experimentum idem, ſed denudabam vitrorum facies ſuperiores; his juncta vitra, nec quatiebant, nec cohæſionem monſtrabant ſatis manifeſtam; ſejuncta quatiebant ambo, ſed tenuius validius; iterum juncta dudum attrectabam, atque tum a divulſione vitrum tenuius erat utrinque electricum exceſſu, craſſius utrinque defectu; inverſo tenuiore, electricitates mutabantur in contrarias; inverſo alio manebant eædem.

XXVIII. Porro in primo experimento vitrum, quod contrarias electricitates acceperat in faciebus oppoſitis, quodque adeo habebat vim explodendi, ipſum erat præpollens; in experimento altero, quod ſejunctum ab alio, aut ſolum explodebat, aut explodebat alio validius, ipſum erat præpollens. Idem

 conſtat

conſtat in tertio hoc experimento; quod nempe vitrum explodit alio validius, ipſum, quam habet in interiore ſua facie electricitatem, eandem determinat in facie ſua exteriore, & contrarias determinat in faciebus ambabus vitri ſocii (XXVI, XXVII.)

XXIX. Pergebam experiri vitris iiſdem ſimiliter electricis a catena, ſed unius ſuperiorem, alterius inferiorem faciem denudabam; hoc illi imponebam, continuo cohæſio obtinebat aliqua; attrectatis extimis induſiis, quatiebar, atque ab hac exploſione cohæſio invaleſcebat; divellebam, & attrectabam ſeorſim ſingula vitra, vitrum tenuius quatiebat me mediocriter, aliud non item: juncta faciebus iiſdem contrariis cohærebant aliquomodo. Quare cum in experimento hoc tertio aptantur altera alteri contrariæ vitrorum facies, ſimilia iis obveniunt, quæ in experimento ſecundo, ubi duo vitra fiunt junctim electrica.

XXX. Cæterum quales in ſecundo, tales in hoc experimento contingunt poſt exploſionem electricitatum oſcillationes, præſertim cum experior, ut in num. XXIX. tum quæ pendent a ſola vitrorum conjunctione, & disjunctione, tum quæ cum iis ſeſe componunt, & efficiuntur ab inverſione vitri præpollentis.

XXXI. Vitri autem præpollentis (XXIX) vim aliter etiam experiebar. Poſteaquam jam ſemel ſejuncta adegeram ad exploſionem, iterum jungebam, tum iterum disjungebam, atque vitrum tenuius iterum indutum explodebat aliquomodo; quod denuo, & tertio adhuc experiens aliquam adhuc percipiebam commotionem: vitrum autem craſſius,

7 quum

quum in ipſo ſimiliter experiri pergerem, nullo me unquam commovebat modo.

Experimentum quartum.

XXXII. Hactenus de vitris duobus; pauca nunc addam de uno. Vitrum rite indutum, & electricum a catena ſummo prehendens angulo ſuſpendo, tum diverſa conſector, quæ exiſtunt phænomena, induſia attrectando, divellendo, divellendo digitis, aut divellendo ſtaminibus ſericis; eaque omnia experior tum ante, tum poſt exploſionem. Itaque, ſi alternatim attrecto induſia vitri electrici, alternatim ignem haurio a facie redundante, immitto in carentem; & copia ignis quaque attrectatione hauſti, aut immiſſi reſpondet ſummæ capacitatis [e] induſiorum, et magnitudini electricitatum abſolutarum reſiduarum; eaque lege ignis hauritur, & immittitur alternis his attrectationibus, ut conticeſcat electricitas omnis in facie attrectata, reviviſcat in facie oppoſita.

XXXIII. Si induſia oppoſita attrecto ſimul ambo, exploſio fit; intereaque induſia vitro adhærent va-

[e] Ceu ſi induſium faciei unius vitri communicet cum uno homine ſejuncto a ſolo, aut cum duobus, aut cum tribus ſimiliter ſejunctis, aut ſi communicet etiam induſium faciei alterius cum uno, duobus, tribus, &c. hominibus, aut corporibus aliis deferentibus: copia ignis, quæ alterna quaque attrectatione immittitur in faciem carentem, aut hauritur a redundante, proportione reſpondet ſummæ capacitatis & induſiorum chartaceorum, & hominum, aut corporum aliorum, quæ cum una, aut ambabus faciebus communicant; atque prout major eſt ſumma earum capacitatum, minore attrectationum numero exhauriuntur abſolutæ oppoſitarum facierum electricitates.

lidius; & dum pergo attrectare, explosio, & adhæsio consummantur.

XXXIV. Pergo nunc indusia divellere staminibus sericis, ut, si quam ipsa a divulsione habent electricitatem, retineant; atque ante explosionem facies quæque vitri ea cautione denudata electricitatem ostendit sibi contingentem, indusium quodque electricitatem pariter respondentem demonstrat, & suæ capacitati proportionalem [f]; dico autem contingentes electricitates, quæ datæ faciei ante explosionem conveniunt ex theoria Frankliniana.

XXXV. Si post explosionem staminibus sericis indusia divello, facies vitri ambæ electricitatem monstrant, contingentem faciei ultimo denudatæ, indusia demonstrant contrarias; ipsasque iterum suæ capacitati proportionales.

XXXVI. Nunc indusia digitis divello; quæ res non facit solum, ut sentiri non possint electricitates indusiorum, sed aliquam præterea infert mutationem iis electricitatibus, quæ apparent in faciebus nudis, cum per stamina serica indusia divello. Itaque ante explosionem cum digitis divello indusium unum, ambæ facies electricitatem ostendunt eamdem, et contrariam illi, quæ contingeret faciei ultimo denudatæ.

XXXVII. Sed post explosionem cum digitis indusia divello, tum utraque facies electricitatem demonstrat eamdem, sed contingentem faciei ultimo denudatæ; atque hisce in divulsionibus indusiorum, quæ aut digitis fiant, aut staminibus sericis, oscil-

[f] Loquor hic de capacitatibus, quas habent indusia chartacea sola; etenim unice circa eas capacitates expertus sum.

lationes

lationes electricitatum aliquæ facile observari possunt. Unam hic ego attingam, quæ accidit in hac ultima experimenti hujus parte. Vitri, quod adegi jam ad explosionem, latus unum apprimo stomacho meo, læva prehendens latus oppositum, tum dextera divello indusium inferius, continuo pollice lævæ admoto indusio superiori, existit scintillula; divulsum indusium inferius iterum admoveo, superius scintillam exhibet aliam, sed primæ contrariam; iterum illud divello, iterum hoc scintillat, uti vice prima; illud rursus admoveo, rursus hoc scintillat, uti vice altera, &c. scilicet, si alternatim divello, & admoveo indusium faciei defectu electricæ, in divulsione indusium superius accipit ignem, effundit in admotione; si divello indusium faciei, quæ ante explosionem erat excessu electrica, indusium aliud inter divellendum effundit ignem, accipit inter admovendum. Quæ omnia penicillo, & stellula, aut motibus electricis confirmavi.

XXXIII. *Specimen Hiſtoriae Naturalis Volgenſis.* *Auctore* J. R. Forſter.

Read June 18, 1767.

ANTEQUAM de quolibet genere corporum naturalium circa Volgam flumen agere poſſim, neceſſarium duxi quaedam circa loca & ſitum locorum praemonere.

Regio cujus hiſtoriam naturalem ſum deſcripturus, eſt inter gradum 52 & 48 latitudinis borealis, eſtque ſita ex utraque ripa fluminis Volgae; id flumen ad Saratoviam circiter milliaris Anglici latitudinem habet; at tempore verno, dum nives per univerſam Ruſſiam diſſolvuntur, aqua ingens incrementum capit, & non raro ſeptem novemve pedes Anglicanos ſurgit, & tum temporis in ripa orientali per duorum triumve Anglicorum milliarium ſpatium diffunditur, et uſque ad altiorem ibi inſurgentem planitiem ingentis deſerti late vagatur, prataque & ſilvas ibi foecundat. A menſe autem Maio uſque in Novembrem vel initia Decembris, quo ſcilicet tempore gelu conſtringitur, flumen ſenſim decreſcit. Omnis ripa occidentalis Volgae per pluſquam 260 milliaria Anglica eſt montoſa; montes aſſurgunt in littore a 30 ad 60 orgyas Ruſſicas, quarum quaelibet eſt 7 pedum Anglicorum; inde duorum triumve milliarium Anglicorum ſpatio a flumine, alia ſeries montium, paene ejuſdem altitudinis procurrit, eodem tenore quo flumen: & in horum montium dorſo frequens eſt ſilva plurimum ex pulcherrimis conſtans quercetis ab ipſo iugo horum

horum montium ingens planities, protenditur, ad usque flumen *Khoper* (Choper) quod in Tanaim influit nisi qua, hinc inde, rivis quibusdam dividitur. Omnis superficies harum regionum tecta est ad duorum triumve pedum altitudinem, terra admodum nigra, pingui & ad miraculum usque fertili; sub ea nil nisi marga & lapides margacei cretaceive reperiuntur, ad ipsum usque flumen Volgam. Haec eadem ripa occidentalis innumeris intersecta est convallibus, per quas rivuli & fontes, quorum pauci sunt perennes, ex propinquis defluunt montibus. In omni hac regione gramina & variae plantae primo vere ad duos tresve pedes rapidissime crescunt. Ingravescente aestu sensim siccantur herbae in vastis campis, & dein ab incolis, qui rari admodum reperiuntur, primo vere igni comburuntur, maximo regionis detrimento; quia humiditas ex solo expellitur, & terrae crusta veluti testacea inducitur; ut taceam salia, quae hinc in hac ipsa terrae incultae superficie, nimia sunt, & hyeme certe frigus intendunt, aestate vero nimiam procreant siccitatem; praeterea & totae silvae flammis vento agitatis non raro intenduntur & pereunt maximo damno regionis hujus adeo frigidae.

Ripa orientalis Volgae plane novum orbem nobis sistit. Ab ipso flumine per duo triave milliaria Anglica littora cincta sunt pratis amoenissimis, & silvis, fruticetisque varii generis: innumeri rivuli ex Volga derivati, paludes & lacus, haecce prata intersecant. Dein supra haec prata ad 30 pedum altitudinem, colles sunt ascendendi, ex quorum summo vastus campus & planities immensa ad Yaiki usque ripas protenditur ad 130 vel 150

milliaria Anglica. Solum in pratis ex argilla, arena, marga, & putrefactis vegetabilibus constat. Planities superior nil nisi mera argilla est, & maxime tritici miliique ferax. In medio harum regionum est planities arenis cooperata & ericetis, in qua si vix ad pedis unius alteriusve altitudinem arenam eruere volueris, ingentem vim habebis aquae dulcis: praeterea innumeris in locis sunt lacus aquae salsae, rivulique amarulentam salsamque habentes aquam; imo & plures illic sunt lacus qui stratis salis muriatici 3 circiter pollicum crassitudine sunt lecta, ad plusquam decem duodecimve pedum altitudinem; & si hic inde rivuli aquae dulcis per valles quosdam defluunt, non tamen ubique aqua supra terram fluit, sed saepius per arenas & margacea strata transit, & post aliquot demum milliarium intervallum, denuo fluere incipit.

Coelum his locis plerumque sudum & serenum est. Hyeme intensissimum frigus, multaeque decidunt nives, circa mensis Novembris finem & per Decembrem: at fine Martii vel initio Aprilis subito, aëris temperie nives liquescunt herbae ubique luxuriant, & brevi tempore ad cubiti altitudinem succrescunt, & inde uno tenore aër usque ad summum & poene intolerabilim aestum incalescit, adeo ut gradum 103½ thermometri Fahrenheitiani celeberrimus Lerchius Astrachani observaverit: & ego circiter sub gradu 51 degens mensibus Maio, Junio, & Julio 1765 in thermometro de Lisliano gradum 97 observavi, qui circiter cum 93 ½ thermometri Fahrenheitiani coincidit.

Praeterea id precor cuivis legenti sequentes observationes animo obversetur, me non tam voluisse omnis

omnis numeris perfectam historiam naturalem harum regionum scribere; huic enim operi, nec tempus brevissimum, quo in iisdem oris mihi versari contigit *, nec vires, ob reliquas mihi demandatas maximi momenti occupationes, nec ipse parvus tenuisque, quod scio ingenii mei modulus suffecissent; sed potius primitias historiae naturalis, me orbi erudito, ea qua par est animi modestia, offerre.

Naturae sequar ordinem in exponendis rebus; ab inanimatis, ad vegetantia indeque ad viventia processurus, in plantarum denominatione in animalium dispositione celeberrimi Linnaei editos libros, methodumque undique receptam, adhibiturus sum.

Regnum Minerale.

(A.) TERRAE.

A. Argilla.

α *Coloris lutei.* Omnis regio trans Volgam, quam late desertum inter Volgam Gaïkum, Samaram & mare Caspium patet, nil nisi hujus generis argilla est, at tritici miliique feracissima. Eadem argilla hinc inde & ex hac parte Volgae reperitur prope Saratoviam, prope coloniam Palatinam, ut & prope colonias ad rivulos dictos *Lesnoi Karamysh*, *Sosnofa*, *Ylafla*, & *Koola lynka.*

β *Coloris rubicundi*, particulis ferri impraegnata prope Volgam ad rivulum *Chmielofka*, ut & circa *Tarlyki* minoris ripas trans Volgam.

* Scil. à mense Maio ad initium Sept. 1765.

γ *Rhomboïdalis lamellosa lutea.* Haec argillae species in torrente qui Saratoviam urbem transit, in determinato profundo reperitur.

δ *Rhomboïdalis lamellosa nigricans.* Tactu mollissima pinguis & laevis sub argillae num. γ. memoratae strato invenitur eodem in loco; cum acidis effervescit, & plurima testacea in ea reperiuntur.

B. MARGA.

α *Viridescens, pinguis,* in colonia Palatina sub humo nigricante, reperitur maxima in copia.

β *Marga cinereo albicans cretacea,* ibidem eodem in loco effossa.

C. CRETA.

Circa villam Zolotoi prope Volgam reperiunda; incolae ea utuntur ad dealbandos parietes.

D. HUMUS NIGRICANS PINGUIS.

Cis Volgam omnis late regio a Pensa urbe ad Tanaïs viciniam ad duorum triumve pedum altitudinem hac humo tecta est: & uti facile ex adspectu dijudicari potest, orta est ex vegetabilibus per innumeras annorum series, in his locis quotannis putrefactis.

E. TERRÆ MINERALES.

α *Arena martialis,* prope *Ylastam* in vicinia Canalis ab Anglo Perry effossi, ad jungendum Tanaïm Volgae, item prope *Medveditzam,* eamque influentes rivulos *Tersam, Dobrynkam,* & *Burluk.*

β *Terra sale & hepate sulphuris mixta* in fundo lacus Yeltoniensis salsi, saepe inter strata salis reperitur.

γ *Argilla sale communi impraegnata.* Cis Volgam prope Saratoviam, ad montes Accipitrinos (*sokolowe gori* vulgo) & trans Volgam in ripa minoris Tarlycki & plurium minorum rivulorum salsam aquam vehentium, statim ex magno proventu Salicorniae, Salsolae & Anabaseos dignoscitur.

δ *Terra selenitica*, in torrente qui urbem Saratoviam transit in *argilla rhomboïdali, lamellosa, nigricante* reperitur.

F. Arena.

α *Glarea mobilis* ad littora Volgae orientalia praesertim reperitur.

β *Glarea argillosa*, hinc inde in pratis, trans Volgam.

γ *Sabulum particulis minoribus*, spathi, quarzi, & micae compositum. In medio vasti deserti trans Volgam, campus ingens hac arena coopertus in longum 140 in latum 50 milliaribus patens, & *Ryn* appellatus, a Volga 100, Yaiko 60 circiter, a mari Caspio autem 120 milliaribus Anglicis abest.

(B.) LAPIDES, in universum admodum rari.

α *Quarzosi*, in summis jugis montium cis Volgam reperiuntur integrae rupes ex hoc lapide compositae. Magnae satis molis & nomine aurium (*Ushy*) appellatae, prope Dmibresok urbem, trans Volgam, ultra 30 mill. Angl. conspicuae sunt.

β *Cosarenarius* in summis montium jugis ibidem.

Calcarius,

γ *Calcarius*, petrefactis testaceis plenus, in praerupta Volgae ripa prope Saratoviam, in strato duorum circiter pedum, a summo montis jugo 10 circiter pedibus.

δ *Lapis margaceus*. Omnia Volgae littora omnesque citeriores partes nil nisi strata hujus lapidis margacei habent, qui nec calci faciendae bonus, nec in aedificiorum usum cedit, quia aëri expositus brevi dissolvitur.

ε *Tophus sale plenus*, in littore lacus Yeltoniensis.

Nota. Trans Volgam ne parvae quidem molis lapides, exceptis his tophaceis, reperies.

(C.) INFLAMMABILIA.

1 *Gagates* 120 milliaribus Anglicis ad Volgae ripam supra *Saratoviam* repertus.

2 *Pyrites sulphureus* purus nudus 26 mill. Angl. supra Saratoviam in australi ripa *Tchardymi* fluminis 6 mill. Angl. a Volga reperitur; & totus mons ex hoc pyrite versus Africum (*S. W.*) ad Kurdyum flumen usque protenditur. Decem librarum pondo, dant per destillationem 16 libras sulphuris residuum vitriolo martis, & partibus martialibus est impraegnatum.

(D.) SALIA.

1. *Sal commune*.

Ingentem salis copiam maximae Russici imperii parti sufficientem praebet lacus *Yelton* a Russis, a Khalmuccis autem *Gelton-noor*, appellatus. Is

Is lacus trans Volgam a Saratovia 160, a Dmitrefok urbe circiter 80 Angl. mill. abeſt. A campo arenoſo Ryn ſupra memorato 20 circiter mille paſſibus remotus; 10 in latum & ſedecim in longum, circuitu vero mill. Angl. patet. Plures in eum incidunt rivi ſalſi. Aqua vix ultra 4 pedes Anglicos ſuperfuſa, intenſe ſalſa, coloris rubicundi, ab argilla ex montibus ut credo defluente, forte & ab alia cauſa, quam ignorare me fateor: relatum mihi enim eſt, lacum menſe Auguſto, poſt intenſiſſimos calores, maxime rubere. Tertia circiter parte mill. Angl. a littore incipiunt ſtrata ſalis per omnem lacum extenſa, 3 circiter pollices craſſa; haec ſtrata in medio lacu ad inſignem altitudinem reperiuntur. Anno 1746, quum primum hoc ſale uti inciperent, curribus per lacum uti per glaciem commeabant, ſalem vecturi: ſed curioſius inquirentes, quotnam ſtrata ſalis in lacu eſſent, tandem aquae ſub ſtratis incluſae locum dedere prorumpendi, & ex eo tempore aqua lacui ſuperfuſa eſt.

Mille vel 1200 homines lintribus circiter 400 vel 500 a fine Maii menſis, in altum Angl. circiter milliare a littore evecti in aquam deſcendunt & ope ferri conto impacti, ſtrati ſalis partem aliquam a reliquo revellunt, trium quatuorve pedum plerumque magnitudine, & in lintrem tollunt, dein lintre onerato unus in littus cum ſale abit, reliqui alium lintrem replere conantur. Poſtero die antequam ad laborem redeant, ſalem in littore malleis ligneis comminuunt, & aqua lacuſtri eluunt,

eſt

est enim maximopere impurum. Quotannis ad 120 millena millia librarum salis a Maio ad Augustum eruunt, & 6000 vel 8000 bobus Saratoviam & Demetriopolim (Dmitrefok) ad Volgam in Imperatricis devehunt horrea, praeterea innumeri equi, carris juncti, salem in usum populi in vicinia exportant. Pro 40 libris salis, pro labore, operariis, & iis qui salem vehunt in universum Saratoviae $4\frac{1}{2}d$. Demetriopoli (Dmitrefok) tantum $2\frac{1}{4}d$. monetae Anglicae ex publico rependunt. Deinde autem salem per omne imperium Russicum navibus distribuunt & carris. Tum vero omnes unam libram $\frac{1}{2}d$. Angl. pretio, emere possunt, ex publicis horreis, quovis in loco. Ingens copia *salis mirabilis*, inter strata salis passim reperitur, & licet maximam ejus partem supradicto modo eluant, remanet semper aliqua ejus pars, quae efficit, ut hoc sale minus bene in aspergendis utantur carnibus, praesertim in usum nauticum, ideo in imperatoriae classis usum quotannis Hispanum important salem. Praeter hunc lacum sunt & plures alii sale pleni, versus mare Caspium, in vicinia Astracani; sed eo sale uti vetitum est, ne in detrimentum publicorum redituum eum eruerent; id vero inde incommodum ortum, ut piscatui nunc minus sint intenti populi Caspio mari vicini, salis nempe pretio duplo majori, quam ante fuerat, facto.

2 *Sal mirabile* uti supra memoravimus inter strata salis communis reperitur, in eodem lacu Yeltonensi;

2 3. *Alumen*.

Alumen.

Urbem Saratoviam transit rivulus in praerupta valle, in eundem a parte septentrionali influunt fontes tres, quorum aqua saporis est maximopere adstringentis; cum ea quaedam experimenta institui, ut nempe potui; non enim in hisce oris in promptu sunt omnia ad ejusmodi experimenta rite instituenda.

α *Odorem* nullum in hac aqua observare potui.

β *Visui* aqua se offert limpidissima; nec per complures dies asservata mutationem ullam subiit.

γ *Sapor* intense stypticus, adstringens, amarulentus.

δ *Media aestate*, frigidissima erat dum ex fonte prodiret.

ε *Solutio argenti in* 🜄, aqua diluta, & huic aquae affusa, eandem subito turbidam, mox lactei prorsus fecit coloris: mox coagulatae in aqua videri poterant partes, quae sensim subsidere videbantur; superior pars hujus coaguli nigricantis erat coloris.

ς *Affuso Sp. Sal. Amoniae*, similiter coagulum in aqua videri poterat.

ζ *Ferrum politum & laevigatum* aquae injectum nigrum induit colorem, & in summa aquae superficie, ferro adhaesit ochra.

θ *Argentum laevigatum*, aquae immissum nigrum induit colorem, praeterea splendorem exuit.

η *Solutio* ♀ *in* 🜄 affusa, coeruleo colore eam tinxit, praeterea vero nil mutatum.

ι *Albumen ovi* aquae immiſſum nil mutavit.

κ Tinctura quaedam *coerulea*, mihi falſo pro *Syrupo* violarum vendita, (erat enim tinctura Flor. Aquilegiae, aquae admixta,) nullam ſubiit mutationem.

λ *Solutio Mercur. ſublimati* aquam aeque limpidam ut antea reliquit.

μ *Infuſione Gallarum* affuſa, nil mutatum.

ν *Solutio Sal. Tartari* aquae adfuſa lacteum colorem ei induit, dein & coagulatae partes viſae.

ξ *Solutio Sachar. Saturni* aquam minus limpidam fecit, & poſtero die partes coagulatae viſae.

ο *Solutio Aluminis* affuſa nil mutavit.

Ex hiſce ſatis apparet Aluminis aliquantum huic aquae ineſſe; praeterea viſum eſt mihi partes ſulphureas aquae admiſtas eſſe; ſed quia plura experimenta inſtituere mihi non licuit, utique aquae hujus reliqua ingredientia & proportionem obſervare nequii.

4 *Vitriolum Martis* in Pyrite ſupra deſcripto lit. C. n. 2. ineſſe, certiſſimum indicium fecit lixivium reſidui poſt diſtillationem ♀ris evaporatum.

(E.) MINERAE.

In omni hac regione hactenus nonniſi ferri minerae ſunt inventae.

a *Minera ferri cavernoſa*, cujus 40 librae dant 16 libras ferri optimi, magis ſcoriae quam ferri minerae ſimilis, at metalli dives eſt, & facillime liqueſcit, adeo ut eo gradu ignis, quo aliae minerae vix calcinantur, haec noſtra jam liqueſcat.

In

In ſuperficie terrae inter arandum copioſe reperitur in agro coloniae Palatinae. In deſerto verſus Choper fluvium magna in copia obvium eſt Ignem certiſſime jam paſſa eſt minera, ideo forte a monte ignivomo quondam eructata.

b *Minera ferri lamelloſa, mixta arenae ferrariae,* ex fodinis ferri, circa montes, qui imminent rivulis, *Terſa, Dobrynka,* & *Burluk.* Ferrum dant durum & non magni pretii, ex minera refractaria. Ejuſdem generis minera ferri, circa foſſam reperitur, quam Anglus Joannes Perry, juſſu Imperat. Petri I, ad jungendam Volgam & Tanaim duxit.

(F.) PETREFACTA.

Plurima Ammoniorum, Mytulorum, Terebratularum, Chamitarum, & Pinnae marinae exemplaria circa Saratoviam in lapide calcario & in argilla teſſelata nigra ſupra deſcripta reperiuntur.

REGNUM VEGETABILE.

Plus quam ſeptingentas in itinere meo collegeram herbas; ſed plauſtris, quibus cum reliquis ſarcinis & herbaria mea impoſita erant, aquam profundam tranſeuntibus, ſemina & herbaria quoque madefacta ſunt, nec primis ſtatim obſervavi diebus, dein irreparabilem jacturam me paſſum eſſe vidi, omnes enim plantae mucore corruptae, prorſus abjiciendae erant: & cum in chartas haſce bibulas, pro reſtaurando damno alias collegiſſem herbas, & has mucor ex charta propullulans infecit, itaque nonniſi reliquias

 harum

harum plantarum & paucas bene conſervatas retuli, ex itinere. Hinc cuilibet patebit nonniſi ſpecimen, admodum puſillum heic florae Volgenſis offerri. In determinandis plantarum nominibus, Clariſſ. Falckius, Prof. Botanices in Horto Pharmaceutico Imperiali, pro inſigni amicitia qua me proſequitur, me plurimum adjuvit, quod grata admodum mente publice fateor.

DIANDRIA MONOGYNIA.

1. SALICORNIA *caule geniculato*; in apicibus ramorum ſunt clavae floriferae articulis cylindricis, ex cujuſvis articuli commiſſura prodeunt ſex flores; in cavitate rotunda, prominet ſtylus, & adſunt duo ſtamina cum antheris.

2. ——— *caule tereti ſine geniculis*; in ramorum apicibus ſunt clavae floriferae articulis tetragonis rhomboidalibus; ex cujuſvis articuli commiſſura prodeunt quatuor flores, ad rhomborum apices; ſtamina cum antheris duo, ſtylus minimus.

Nota. Quia antherae ob ſtaminis tenuitatem ſunt maximopere flexae, ipſe humi poſitus oculo armato innumeros flores accuratius obſervavi, & in omnibus, duo ſtamina & unicum ſtylum reperi: itaque hae ſalicorniae ſpecies differunt, in his regionibus ab iis, quas alii Botanici alibi cum una vel pluribus obſervaverunt antheris. Florent menſe Julio & Auguſto in locis humidis, prope lacus & rivulos ſalſos trans Volgam ad lacum Yelton, & in vicinia Yerooſlani fluminis.

3. VERONICA *incana*, foliis oppoſitis ſuperioribus ſeſſilibus, integerrimis, inferioribus & radicalibus crenatis.

crenatis & petiolatis. Floret menſe Junio locis ſiccis montoſis cis Volgam.

4. ——— *longifolia* floret a Julio in Auguſtum ubique cis Volgam in campis & ſylvis.

5. GRATIOLA *officinalis*, floret a Julio in Septembrem in pratis & locis humidis, ubi ſeſe per ſtolones reptantes propagat.

6. SALVIA *nemoroſa* floret a Maio per totam aeſtatem locis aridis.

TRIANDRIA MONOGYNIA.

7. VALERIANA *officinalis* floret m. Jun.

8. IRIS *pſeud. Acorus* floret m. Junio in aquoſis.

9. —— *Sibirica* floret m. Maio & Junio in pratis humidis ad Volgam.

TRIANDRIA DIGYNIA.

10. PHALARIS *erucaeformis* floret Jun. in campis cis Volgam.

11. STIPA *pennata*, ariſtis longiſſimis floret m. Junio, ſeqq.

TETRANDRIA MONOGYNIA.

12. SCABIOSA *arvenſis* floret m. Julio ſeqq.

13. ——— *ochroleuca* floret in campis & ad ſegetes m. Jul. ſeqq.

14. GALIUM *rubioides* floret m. Junio.

15. ——— *paluſtre* floret m. Junio circa rivulos in ſilvis.

16. ——— *verum* ad rivulos & fontes m. Jun.

17. ——— *Aparine* ad ſepes villarum m. Jun.

18. RUBIA

18. RUBIA *peregrina* foliis quaternis; baccae duae monospermae, femina umbilicata; floret m. Jun. ad ripam Dobryn, Kae & Yerooslani.
19. PLANTAGO *major*, ad vias & semitas passim, fl. m. Jul.
20. ——— *media*, in montibus soli expositis, fl. eodem tempore.
21. SANGUISORBA *officinalis*, copiose in pratis, fl. m. Jun. seqq.
22. ALCHEMILLA *vulgaris* in pascuis humidis.

TETRANDRIA DIGYNIA.

23. ANONYMOS calyx exterior triphyllus, interior quadriphyllus. Corolla nulla, styli duo plumosi, antherae majores sagittatae, foliola linearia fasciculata, flores alares, radix lignosa ut & tota planta, quae raro pedem & dimiduum excedit. Ob seminis defectum determinare non potui, quo potissimum sit referenda haec planta; floret Julio & Aug. locis aridis in deserto trans Volgam prope Yerooslanum flumen.

PENTANDRIA MONOGYNIA.

24. MYOSOTIS *scorpioides* locis humidis floret m. Junio.
25. ——— *apula* fol. hispidis, racemis foliosis locis aridis m. Jul.
26. LITHOSPERMUM *officinale* floret Maio in montibus & locis apricis circa Koordyoom & Saratoviam.

27. ONOSMA,

27. ONOSMA, *simplicissima* sol. lanceolato—linearibus, confertissimis, asperis, antheris sagittatis, quae mihi connatae videbantur; fl. m. Jun. locis altioribus siccis.
28. ECHIUM *Italicum* flor. m. Junio in collibus.
29. LYSIMACHIA *vulgaris* floret m. Junio locis humidis.
30. ——————— *nummularia* floret m. Augusto in pratis.
31. CONVOLVULUS *arvensis* fl. m. Jul. inter segetes passim.
32. ——————— *sepium* inter sentes & dumeta fl. Julio.
33. POLEMONIUM *coeruleum* in sylvis & pratis floret m. Maio.
34. CAMPANULA *rapunculoides* habitat in montosis regionibus fl. m. Junio.
35. ——————— *Trachelium* in sylvis fl. eod. temp.
36. VERBASCUM *Thapsus* fl. m. Jun. locis siccis.
37. ——————— *Lychnitis* flor. m. Jun. in campis.
38. ——————— *nigrum* ad ripas rivulorum & fontium m. Jun. seqq.
39. ——————— *Phoeniceum* florib. odoratissimis fl. m. Maio & Junio in campis desertis prope Petrofsk, & prope Sosnofkam in Volgae ripa.
40. SOLANUM *Dulcamara* fl. m. Junio locis umbrosis.
41. ——————— *nigrum* fl. in Jun. in arvis & ad sepes.
42. RHAMNUS *Frangula* fl. m. Maio in humidis ad rivulos.
43. EVONYMUS *Europaeus* fl. m. Maio in sylvis ad rivulos.

44. RIBES *nigrum* fl. m. April. & Maio in sylvis passim.

PENTANDRIA DIGYNIA.

45. ASCLEPIAS *Vincetoxicum* locis humidis & umbrosis fl. m. Jun. seqq.
46. SALSOLA *Kali* foliis subulatis, spinosis, caule decumbente, calycibus axillaribus, marginatis.
47. ———— *rosacea.*
48. ———— *prostata.*
49. ———— *hirsuta.*
50. ANABASIS *foliosa.* Omnes hae plantae reperiuntur ad rivulos & paludes salsas, & locis sale communi imbutis, prope Saratoviam & trans Volgam in deserto prope lacum Yelton & rivulos salsos in hunc lacum sese exonerantes; fl. m. Julio.
51. ULMUS campestris in sylvis passim.
52. ERYNGIUM *planum* ad vias passim fl. m. Julio.
53. HERACLIUM *Sphondylium* in agris & campis passim fl. m. Jul.
54. ANGELICA *atro-purpurea* floret m. Jun. locis humidis.
55. ———— *sylvestris* floret m. Junio in umbrosis passim.
56. CHAEROPHYLLUM *sylvestre* fl. m. Jul. in umbrosis.
57. AEGOPODIUM *Podagraria,* in sylvis & umbrosis passim fl. in Junio.

PENTANDRIA TRIGYNIA.

58. VIBURNUM *Opulus* in sylvis & humidis floret m. Junio.

PEN-

PENTANDRIA PENTAGYNIA.

59. STATICE *reticulata* } florent m. Jun. locis
60. ——— *ſpecioſa* } aridis & paſſim ſal-
61. ——— *tatarica* } ſugine aſperſis.

62. LINUM *perenni* floret m. Jun. & Jul. in pratis prope Coloniam Palatinam.

HEXANDRIA MONOGYNIA.

63. ALLIUM *paniculatum*, floret m. Jun. in locis deſertis.

64. FRITILLARIA *Meleagris*, flor. April. in montibus aridis ad Volgam.

65. TULIPA *ſylveſtris*, flor. m. April.

66. ——— *Geſneriana* ——— locis aridis pulcherrimis coloribus nonnulla exemplaria ſuperbire vidi, praeſertim ad tumulos quoſdam ſepulchrales in medio deſerto 60 circiter mill. Angl. ab omni habitatione remoto.

67. ASPARAGUS *officinalis*, in omnibus pratis ad Volgae ripam copioſe fl. m. Jun.

68. CONVALLARIA *maialis* flor. in ſylvis m. Maio.

69. ——— *multiflora*.

70. ——— *bifolia*, fl. m. Maio in humidis.

71. ACORUS *Calamus*, in aquoſis & paludibus paſſim.

72. RUMEX *Acetoſa*, in pratis Jun.

HEXANDRIA TRIGYNIA.

73. ALISMA *Plantago aquatica* in paludibus ad Haſlam & trans Volgam, in pratis m. Jul.

 HEP-

HEPTANDRIA MONOGYNIA.

74. TRIENTALIS *europaea*, fl. m. Maio in ſylvis,

OCTANDRIA MONOGYNIA.

75. EPILOBIUM *anguſtifol.* } florent m. Junio lo-
76. ——————— *hirſutum*, } cis humidis.
77. ——————— *tetragonum*, }
78. VACCINIUM *Myrtillus* in ſylvis humidis m. Maio fl.
79. ——————— *Oxycoccos* in paludibus frequens; & modo non omni tempore venum, aſportantur baccae Oxycocci, praeſertim m. Jun. Ruſſi eas magna copia ingurgitant, contra calores; vel etiam ſuccum expreſſum, aqua & melle admixtum, pro potu habent, quem palato admodum jucundum eſſe, ipſe expertus ſum.

OCTANDRIA TRIGYNIA.

80. POLYGONUM *Biſtorta* fl. m. Maio in pratis.
81. ——————— *Convolvulus* inter ſegetes paſſim.

ENNEANDRIA HEXAGYNIA.

82. BUTOMUS *umbellatus* in paluſtribus m. Jun.

DECANDRIA MONOGYNIA.

83. ARBUTUS *Uva urſa* prope Twer in paludibus jam in vicinia Volgae fl. m. Maio.

84. PYROLA

84. PYROLA *rotundifolia*, } florent m. Maio in
85. ——— *minor*, } sylvis.

DECANDRIA DIGYNIA.

86. SCLERANTHUS *annuus* fl. m. Maio locis arenosis, radicibus hujus ut & *Potentillae reptantis & Tormentillae* adhaeret *coccus Polonicus*; & a mulieribus frequenter colligitur, aceto necatur, & venum asportatur.

DECANDRIA TRIGYNIA.

87. SAPONARIA *officinalis*, fl. m. Jun. locis siccis.
88. DIANTHUS *arenarius*, fl. m. Jul. & Aug. ad ripas Volgae.
89. CUCUBALUS *tataricus*, } fl. m. Jun. locis aridis
90. ——— *sibiricus*, } & altioribus.
91. ARENARIA *saxtalis*, fl. m. Jun. in montibus siccis ex lapide margaceo constantibus.

DECANDRIA PENTAGYNIA.

92. SEDUM *acre* floret m. Aug. locis arenosis.
93. ——— *Telephium* locis aridis in deserto.
94. AGROSTEMMA *Githago* inter segetes.
95. LYCHNIS *Chalcedonica* prope Coloniam Palatinam copiose in sylvis & locis humidis.

DODECANDRIA MONOGYNIA.

96. PORTULACA *oleracea* ad ripam Yerooslani fluvii.

97. LYTHRUM *Salicaria* ad rivulos fl. m. Jul. Aug. pp.

DODECANDRIA TRIGYNIA.

98. EUPHORBIA *Peplus* fl. m. Julio in agris.
99. ———— *helioscopia* ibid. eod.
100. ———— *palustris* colossea 4 & 5 pedum Angl. ad ripas Volgae & Occae in pratis humidis fl. m. Maio.

ICOSANDRIA MONOGYNIA.

101. AMYGDALUS *nana*; in campis desertis integrae sylvulae occurrunt, fl. m. Maio.
102. PRUNUS *Cerasus caproniana* in desertis cis Volgam, integrae ex ea sylvae reperiuntur, fl. m. Maio.
103. ———— *Acacia Germanor.* fl. m. Maio inter dumeta.

ICOSANDRIA TRIGYNIA.

104. SORBUS *aucuparia* in sylvis m. Maio.

ICOSANDRIA PENTAGYNIA.

105. PYRUS *Malus sylvestris*; in sylvis hinc inde arbores majores reperiuntur, fl. m. Maio.
106. SPIRAEA *salicifolia* herbacea, ad montium radices, floret m. Jun.
107. ———— *Filipendula* in campis humidis.
108. ———— *Ulmaria* in pratis.

109. SPIRAEA

109. Spiraea *crenata s. tatarica* folia ovato-oblonga, alia apice crenata, alia integerrima, corymbus terminalis; flores albi; fl. m. Maio in campis desertis passim.

Icosandria Polyginia.

110. Rubus *Idaeus* magna ingentique copia in sylvis provenit, & in montibus sylvosis.
111. —— *fruticosus*.
112. Fragaria *Vesca sylvestris* & *pratensis* m. Maio.
113. Potentilia *Anserina* in pratis.
114. ———— *reptans* in campis passim argillosis soli expositis, coccum Polonicum alit.
115. Tormentilla *erecta* in prato sylvestri m. Maio floret, cocci Polonici ferax.
116. Comarum *palustre* in locis humidis.

Polyandria Monogynia.

117. Chelidonium *majus* ad sepes m. Maio.
118. Papaver *Rhoeas* inter segetes m. Junio.
119. Nymphaea *alba* } in flum. Ylafla & in paludibus copiose trans Volgam.
120. ———— *lutea* }
121. Tilia *europaea* copiose in sylvis floret.

Polyandria Trigynia.

122. Delphinium *Consolida* fl. a m. Jun. inter segetes.
123. ———— *elatum* inter fruticeta m. Jul.

124. Aco-

124. ACONITUM *uncinatum*, fol. magnis, palmatis, lacinis magnis, flores pallide coerulei ſingulares in Querceto humido prope Arſmaſſium urbem, exitu m. Maii.

POLYANDRIA POLYGYNIA.

125. ANEMONE *ſylveſtris* fl. m. Maio & Junio in humidis & ſylveſtribus locis.

126. THALICTRUM *flavum* } flor. m. Jun. locis
127. ——————— *minus* } humid.

128. TROLLIUS *europaeus* fl. m. Maio in pratis.

129. CALTHA *paluſtris* ad ripas rivulorum in April.

DIDYNAMIA GYMNOSPERMA.

130. AIUGA *pyramidalis* fl. m. Maio in campis.

131. SIDERITIS *hyſſopifolia* fl. m. Jun. in campis deſert. cis Volgam.

132. MENTHA *aquatica* fl. m. Jul. paluſtribus.

133. GLECOMA *hederacea* in pratis copioſe fl. m. Maio ſeqq.

134. GALEOPSIS *Ladanum* fl. m. Jun. in arvis.

135 PHLOMIS *Herba Venti* fl. m. Jun. in campis.

136. ORIGANUM *vulgare* fl. m. Jun. inter fruticeta.

137. THYMUS *Serpillum* fl. m. Jun. locis ſiccis montoſis.

138. DRACOCEPHALUM *ſibiricum* fl. m. Jul.

139. SCUTELLARIA *galericulata* fl. m. Jul. locis humidis.

Didynamia Angiosperma.

140. Rhinanthus corollis labio ſuperiori, recurvo emarginato; flores pallide lutei, alares in ſpica homomalla, fol. lanceolatis integerrimis, fl. m. Maio & Jun. in campis & ſylvis arenoſis ſubhumidis.

141. ——— *criſta Galli* fl. in pratis m. Jun.

142. Euphrasia *officinalis*, } In campis deſertis fl.

143. ——— *Odontites*, } in Maio & Junio.

144. Melampyrum *arvenſe* inter ſegetes Jul.

145. Pedicularis *comoſa*, flore pallide luteo, m. Junio in campis.

146. Antirrhinum *Linaria* fl. m. Jun. in campis.

147. Scrophularia *nodoſa* fl. m. Jul. ad. aquarum rivulos.

148. Orabanche major fl. m. Jun. in montibus.

149. Dodartia *orientalis*, fol. integerrimis glabris linearibus, flor. caeruleis; fl. m. Julio in campis trans Volgam ad Tarlyk majorem fluvium. Cameli, equi, boves & oves Khalmyccorum ſunt ejus appetentiſſimae, & ad radices uſque demorſam comedunt; ideo magno labore & longiſſima excurſione demum locum intactum nactus ſum, in quo exemplaria integra reperi ex quibus eam definire potui.

Tetradynamia Siliculosa.

150. Alyssum *incanum* in campis fl. m. Jun.

Tetra-

TETRADYNAMIA SILIQUOSA.

151. SISYMBRIUM *Sophia* fl. m. Jun. in aridis.

MONADELPHIA POLYANDRIA.

152. SIDA; *calyx* tomentoſus, coriaceus, quinquefidus, complicatus, *folia* magna, cordata, crenata, acuminata, tomentoſa, petiolis folio longioribus; capſulae tomentoſae, 12, 13, 14, calyce longiores, ſemina in qualibet capſula tria, reniforma, fl. m. Aug. ad Yerooſlani ripam.
153. ALTHAEA *officinalis* flor. m. Jun. in campis humidis.
154. LAVATERA *thuringiaca* fl. m. Jun. inter fruticeta.

DIADELPHIA OCTANDRIA.

155. POLYGALA *vulgaris* } fl. m. Jun. in campis herboſis.
156. ——— *ſibirica* }

DIADELPHIA DECANDRIA.

157. SPARTIUM *complicatum* fl. m. Maio locis ſiccis.
158. GENISTA *tinctoria* fl. m. Jun. & Jul. in campis herbidis patentiſſimis.
159. LATHYRUS *tuberoſus*, } fl. m. Junio copioſo in pratis & campis.
160. ——— *pratenſis*, }
161. VICIA *piciformis*, ſtylus villoſus petiolis anguloſis ſubſtriatis, ſtipulis ſinuato-ſagittatis, foliis ovatis, retuſis, mucronatis, pedunc. multifloris imbricatis,

catis, flores ochroleuci; crescit inter dumeta in ripa rivuli in Colonia Palatina.

162. —— *dumetorum*, inter fruticeta, } m. Jun.
163. —— *Cracca* in pratis, }

164. GLYCYRRHIZA *echinata* fl. m. Jun. ad ripam Volgae & Dobrinkae in pratis.

165. —————— *glabra*, integros ubique ac spatiosissimos campos occupat, fl. m. Jun.

166. CORONILLA *varia* fl. m. Jun. in campis & pratis.

167. HEDYSARUM *Onobrychis* fl. m. Jun. in montibus margaccis.

168. PHACA *alpina* fl. m. Jun. in campis.

169. ASTRAGALUS *contortuplicatus* m. Jun. in campis siccis.

170. —————— *pilosus* in aridis locis fl. m. Julio.

171. —————— *Syriacus*, in locis altioribus.

172. TRIFOLIUM *Melilotus officinalis*, toti campi meliloto, fl. luteo & albo, occupantur.

173. —————— *rubens* fl. m. Jun. in pratis.

POLYADELPHIA DIANDRIA.

174. HYPERICUM *perforatum*, inter fruticeta.

SYNGENESIA POLYGAMIA AEQUAL.

175. SONCHUS *Alpinus* in campis altioribus fl. m. Jun.

176. LEONTODON *Taraxacum* in pratis m. Maio.

177. CICHORIUM *Intybus*, fl. m. Jul. in campis.

178. ARCTIUM *Lappa* fl. m. Jun. ad sepes.

179. EUPATORIUM *cannabinum* fl. m. Junio ad aquarum rivulos.

SYNGENESIA POLYGAMIA SUPERFLUA.

180. TANACETUM *vulgare* m. Jul. in campis & ad ſepes.
181. ARTEMISIA *Abrotanum* ad Volgae & Occae ripam copioſiſſime.
182. ———— *Pontica* fl. m. Aug. in campis aridis.
183. ———— *Abſynthium* in montibus ſiccis.
184. ———— *vulgaris* ad ſepes.
185. ———— *Dracunculus* in campis patentibus & ad montium radices.
186. SENECIO *Donia* fl. m. Jun. in pratis.
187. INULA *Helenium* copioſe in pratis & ad rivulos m. Jun.
188. CINERARIA *Alpina*, & quidem ea varietas quam Jaquinus in fl. Vindeb. p. 287. *nudam* vocat, in montibus margaceis ſiccis.
189. ACHILLEA *ptarmica* in pratis trans Volgam copioſe m. Jun.
190. ———— *Millefolium* in campis ad ſegetes & vias paſſim m. Jul.

SYNGENESIA POLYGAMIA SEGREGATA.

191. ECHINOPS *Sphaerocephalus* in campis ſiccioribus.

SYNGENESIA MONOGAMIA.

192. VIOLA *tricolor* paſſim in arvis & pratis trans Volgam.

193. IM-

193. IMPATIENS *Noli-tangere* inter frutices m. Jun.

GYNANDRIA HEXANDRIA.

194. ARISTOLOCHIA *Clematitis* in ſylvoſis m. Maio floret.

MONOECIA TETRANDRIA.

195. BETULA *alba* fl. m. Apr. ſylvar. decus.
196. ———— *Alnus* fl. m. Apr. ad ripas aquarum.
197. URTICA *dioica* fl. m. Jun. ad ſepes.
198. MORUS *Tatarica* habitat in Inſulis Volgae, quotannis inundationi expoſitis circa Tzaritzinum, ubi integrae ejus ſunt ſylvae, ſic & in omnibus inſulis Tanais ab oſtio ejus ad Tzaritzinum uſque reperitur copioſe. *Bombyces* foliis hujus mori educati, ſericum produxere tanti ſplendoris, adeoque bonum, ut nulla in re inferius ſit ſerico Taurinenſi. Imperatrix ipſa ex ſerico a me oblato, juſſit fieri pulcherrimas faſcias, quas capitis ornatus cauſa admovere dignata eſt.
199. XANTHIUM *ſtrumarium* ad ſepes & vias paſſim.

MONOECIA POLYANDRIA.

200. QUERCUS *Robur*. Sylvae ad Volgam & inferiora Tanais ſunt maximam partem quercetum continuum, quod memorandum, quia quercus rarae in ſeptentrionalibus Ruſſiae. Incolae querno ligno in aedificando & in foco utuntur: juniores

quercus piſcatores frequenter caedunt, ut cortice querno, additis veteribus ferramentis, retia nigro tingant colore, magno ſylvarum damno: quamobrem ipſe aliam in Boruſſia uſitatam propoſui methodum retia nigro induendi colore, nempe ut fumigentur, quod & magis conducat retibus, & piſces allicere credatur.

MONOECIA MONADELPHIA.

201. PINUS *ſylveſtris*, hujus arboris ſolas quinque vidi ad villam Soſnofkam, quae hinc & nomen ſortita eſt: praeterea deſunt in ripis Volgae per plus quam 600 mill. Angl. a mari Caſpio.

DIOECIA PENTANDRIA.

202. HUMULUS *Lupulus* arbores ſcandit in locis humidis.

DIOECIA OCTANDRIA.

203. POPULUS *alba*
204. ——— *tremula*
} integrae ſylvae in pratis inundatis, trans Volgam, harum arborum.

POLYGAMIA MONOECIA.

205. VERATRUM *album*. Incolae credunt, id comeſum pecoribus omnis generis mortale eſſe. Radicem ſiccatam & in pulverem reductam, vulneribus in tergo boum inſpergunt, & hac ratione

oeſtrum

oeſtrum expellunt. Copioſe fl. m. Maio in locis humidis circa Saransk & Penſam urbes.

206. Acer *Tataricum* fl. m. Maio ad ripas rivulorum & locis humidis.

Cryptogamia Filices.

207. Marsilea *natans*. Folia plantae ovata, oppoſita, complicata; omnem ſuperficiem foliorum tegunt glomeruli ex quatuor albis filamentis in apice veluti cirrhoideis, compoſiti. Infra ramulos plantae, multae in aquam radices demittuntur; diverſi hinc inde ab his radicibus reperiuntur glomeruli pilorum, qui calicis loco cingunt globulos tres, quatuor, quinque, ſeptem, plures glomeratim adhaerentes, & ſtylis tenuiſſimis inſtructos, globuli maturi continent ſemina plurima albicantia ovata. Itaque nunc credo hac obſervatione me ſatisfacturum deſiderio Ill. *Linnaei*, qui genus Marſileae determinare promiſit, ſi haec Marſileae ſpecies examinata fuerit. Integrae paludes trans Volgam hacce planta ſunt tectae. Quae hic deſcripſi nonniſi oculo armato vidi m. Aug.

Regnum Animale.

Classis prima. Mammalia Terrae.

1. Canis *familiaris domeſticus*, inter ruſticos frequens.

2. ——— *vertagus*, hic uti in omni Ruſſia haec canum ſpecies pilis longiſſimis, & pedibus durioribus

ſubvilloſis, videtur adaptata regioni nivibus hieme opertis, ideoque & tum ad venationem adhiberi poſſunt, quum reliqui vertagi nil proficiunt.

3. ——— *Lupus*, frequenter admodum & pecoribus damnoſi ſunt lupi, hiſce in oris.

4. ——— *Vulpes*, ex utraque Volgae ripa frequentes; ipſe catulum captum vidi trans Volgam ad Tarlyk flumen.

5. FELIS *Catus*. Feles ferae in ſylvis frequentes dicuntur, a me tamen non viſae.

6. MUSTELA *Lutra*. In paludibus & lacubus trans Volgam plurimas Lutras eſſe, relatum mihi eſt: vidi unam, mox ſe aquae immergentem.

7. ——— *Martes*, pelles earum a ruſticis frequenter in urbes venum-afferuntur, non tamen magni faciuntur.

8. ——— *Putorius*, & harum pelles ſaepius venales.

8. URSUS *Arctos*, ad Galkam rivum prope coloniam, paulo ante meum adventum, quinque ſimul ſunt viſi; alius hieme Coloniam Palatinam placide tranſiit, & intactus ſylvam vicinam attigit.

BESTIAE.

10. SUS *Scrofa*, a Ruſticis heic ſatis magnae aluntur ſues. Apri circa Aſtrachanum in arundinetis, prope Caſpium mare admodum frequentes obvii.

11. ERINACEUS *Europaeus*, in ſylvis & montibus reperiuntur.

12. TALPA *Europaea*, reperitur quidem, ſed raro.

Glires.

13. Lepus *timidus*. Heic loci duae ſpecies leporum reperiuntur. *Una* hieme alba, exceptis auribus, aeſtate rufam griſeamque lanam rurſus induit; at haec ſpecies minor eſt, eorumque lana a pileariis uſurpari nequit. *Altera* ſpecies omni tempore aeque ruffo-griſea, ſed major, & lana earum a pileariis uſurpatur. Ex ſola Vorainia, quotannis hujus ſpeciei ultra 60,000 pellium Moſcuam & Petropolim dein devehuntur, unde poſtea navibus ad exteras oras praeſertim in Angliam exportantur.

14. Mus *Marmotta*, in campis deſertis inter Penſam Petroffk & Sarotof urbes ingens Marmottarum copia eſt; cauda piloſa admodum.

15. —— cauda ſubabbreviata, auriculis ſubrotundis, pedibus brevioribus, palmis ſubtetradactylis, plantis pentadactylis, ventre adipoſo terrae adpreſſo, corpore ſubtus pallide luteo, in dorſo griſeo luteſcente, magnitudine excedit Rattum.

16. —— *Rattus*, } in horreis & domibus habitant.
17. —— *Muſculus*, }

Pecora.

18. Camelus *Bactrianus*, tophis dorſi duobus. Una varietas colore fuſco, altera pallide fuſco-albicante; haec rarior. Khalmycci eis utuntur, & pretium eis 12 vel 16 aureor. (*Guinea* vulgo) conſtituunt. Khalmycci & Nogaiji Tartari prope Aſtrachanum ex lana camelorum neta & texta pannos egregios con-

conficiunt, nostro Camelot simillimos, quos ipse vidi; sic & funes optimos ex his lanis torquent.

19. CAPRA *Hircus*.

Tres ejus hic sunt obviae varietates.

1. *Capra vulgaris*, per totam Russiam eadem.
2. *Capra Khalmyccica*, non nisi pedibus brevioribus distincta, ut & totum corpus praecedente multo minus.
3. *Capra Angorensis* in Orenburgensi praefectura apud Vrirgisos passim obvia; sic & curiositatis gratia Astrachani, ex Ghilano Persiae provincia, advectas haberi capras hujus speciei, ex testibus fide dignissimis audivi. Lanas ex Persia advectas & a colonis Germanis, in filum netas & textrinae adhibitas, vidi cum serica.

20. ——— *Sayga* vulgo Russis, relatum mihi eam in campis desertis patentibus frequentem esse; ipse non vidi, his locis; cornua vidi 6 uncias Gallicas paullulum excedentia, teretia annulata, recta in apice paullulum incurvata. Dein Petropoli, animal vidi, circiter duos pedes Parisinos, & quod excedit altum, coloris pallide admodum rufescentis, subtus albicantis.

21. OVIS. *Aries*.

Tres heic sunt varietates ovium.

a *Russica vulgaris* quarum lana, pilis caninis propinquior, non magni pretii; & quod rustici nullam earum curam habent indies lana deterior fit; minores.

b *Khalmyccicae oves*, sunt Russicis multo majores, capita & nares incurvae, aures pendulae, pedes elongati, cauda lata, crassa, adiposa, ultra

ultra 30 libras pendens, lana in adultioribus pilosa rigida, in agnis undulosa splendens; a magnatibus Russiae in vestes hibernas expetuntur pelles agnorum, & magno satis pretio emuntur. Khalmycci pelles has perficiunt lacte, unde id commodi habent, ut non solum sint mollissimae, verum & madefactae & denuo siccatae non rigidae fiant. Oves usque ad 200 libras sunt ponderosae.

c *Tchercassicae oves* lanam Anglicae prorsus aequalem gestant. Hae oves circa Medvedizzam flumen haberi possunt. Quum Imperator Petrus anno 1722 arma in Persiam expediret, & Tchercassicas videret oves, edicto proposito jussit ut per omne Astracaniae & Casaniae regnum hasce oves solas alerent, unde quaedam huc pervenere; mortuo autem paucis post annis Imperatore edictum hocce prorsus neglectum: praeterea ad fluvium Kynel colonia est Tchercassorum, in Orenburgensi praefectura, quae has oves secum adduxit.

22. Bos *Taurus*. Boves Russorum sunt minores, Khalmyccorum majores, maximae ex Vorainia adductae.

23. Equus. *Caballus*.

a *Russici equi* sunt minores, duri & longinquis itineribus apprimis utiles.

b *Khalmyccici Equi*, paullo majores Russicis, duri, imprimis in longinquis itineribus utiles, gramen quodlibet, etsi durum, stramineum, frutices imo aequi bonique faciunt; triduo ad 400 milliaria Anglica cursu perficiunt. Venditi a Khalmyccis non raro per plusquam 100 Angl. milliarium spa-

 tium

tium per deſerta ad ſuos redeunt dominos, ſaepe & Volgam tranant. Hieme ſub nive pede cibum quaerunt.

Nagayenſes & *Turcommannici* & *Baſhkirici* maxime vero *Kirgiſici Equi* multo his ſunt validiores & majores, laboris feraciſſimi, aeque aſſueti ungula ſub nive paſtum quaerere.

d Tchercaſſici Equi, Khalmyccicis vix majores, nec forma praeſtantes, nam cervix eis rigida, & cum plurimis equis ea ſit convexa, his ut plurimum eſt concava, at curſu longe omnium velociſſimi, & aeque ut reliqui laboris feraciſſimi, triduo ultra 500 imo 600 mill. Angl. perficiunt.

Caro equina Khalmyccis pro cibo, pullus inter lautiora reponitur, lac pro potu, aceſcens, fermentans & deſtillatum inebrians, *Kumys*; bis deſtillatum *Arecca* appellatur, ſaporis admodum ingrati.

CLASSIS SECUNDA. AVES.

Accipitres.

1. Falco, *Melanaëtes*, *Aquila*, *Valeria*, cera pedibuſque luteis. Ipſe pullum hujus generis, pluſquam duos pedes Anglicos longum vidi, circa Dobrynkam.

2. ——— *aeruginoſus*, cera luteo-viridi, corpore griſeo fuſco, pedibus luteis, ubique paſſim circa Volgam obvius.

Picae.

Picae.

3. Corvus *Corax*.
4. ——— *Cornix*.
5. ——— *Frugilegus*.
6. ——— *Monedula*.
7. ——— *Monedula tota nigra*, nec unquam caerulescens, occipite incano, fronte, alis caudaque nigris; magnitudine monedulae vel parum minor. Omnes hi *Corvi* in sylvis ad Volgam frequentes.
8. ——— *glandarius*, Pica Glandaria.
9. ——— *Pica*. Pica varia.
10. Gracula *Atthis*, corpore viridi, dorso caerulescente, abdomine ferrugineo, pedibus rubris, magnitudine monedulae; trans Volgam ad Yerooslanum flumen vidi.
11. Cuculus *canorus* in sylvis passim auditur.
12. Picus *martius* } circa arbores putrescentes frequens uterque.
13. ——— *medius* }
14. Merops *Apiaster* gregatim trans Volgam ad Yerooslanum flumen victitat.
15. Upupa *Epops* ad ripas rivulorum in fruticetis prope coloniam Palatinam a me visa.

Anseres.

16. Anas *Cygnus* in lacubus ad Volgam frequens.
17. ——— *Cygnoides orientalis* s. *Anser Moscoviticus* rostro luteo cera magna nigra, occipite atro, sacco gulari; corpus griseo-luridum, albo mixtum, pedes luteo-rubicundi. Passim ut Anser domesticus alitur; tamen multo major est.
18. ——— *Anser*. Anseres feri innumeri in paludibus

trans Volgam degunt. Anſer domeſticus ubique alitur.

19. —— *Anas moſchata* fera & domeſtica, his locis habetur.

20. —— *Boſchas major.*

21. —— *clypeata*
22. —— *platyrhynchos*
23. —— *ſtrepera*
} Hae ſpecies roſtris dilatatis in lacuſtribus circa Volgam ſunt frequentes.

24. —— *Penelope* ſ. *anas fuſca*, capite rufo, fronte alba, cauda acuta in lacuſtribus circa Volgam.

25. ANAS *Querquedula*
26. —— *Crecca*
} Harum ſpecierum innumeri greges in paludoſis trans Volgam habitant.

27. PELECANUS *Onocrotatus*, corpore albo luteſcente in dorſo coloris pallide fuſci. Roſtrum ultra pedem Anglicum longum, inferior mandibula bifurca, cum ſacco gulari, in ripa Yerooſslani 18 vidi.

28. ———— *Carbo* cauda aequali, corpore nigro, roſtro edentulo, in apice incurvato, pedibus nigris tetradactylis palmatis; in occipite cranium in proceſſum oſſeum acutum finitur.

29. COLYMBUS *auritus* in Volga & lacubus vicinis obvius.

GRALLAE.

30. PLATALEA *Leucorodia* coloris albi, pedibus roſtroque, apice rotundato, dilatato, rubris, ad Volgam frequens.

31. ARDEA *Grus* ad Volgam.

32. ——— *Cinerea major* ibidem.

33. ——— *Ibis nigra*, roſtro arcuato, pedibus & roſtro luteo-rubicundis, corpore nigro, virideſcente, magnitudine corvi cornicis, at elatior ob pedes collumque elongatum, longitudo ultra 3 pedes Angl. a roſtro ad pedes; gregatim in paludoſis Volgae degit, ibique ranis, lacertis & ſerpentibus victitat.

34. SCOLOPAX *ruſticola* in ſylvoſis ad Volgam, *the Wood-cock*.

35. ——— *Phaeopus* colore fuſco rufeſcente, pectore griſeo. *The Wimbrel*.

36. ——— *Gallinago*. *The Snipe or Snite*.

37. ——— *minima*. Priori ſimilis, ſed multo minor, plumulis crinibus ſimilibus. *The Jack-Snipe*.

38. TRINGA *Vanellus*,
39. ——— *Glareola*,
40. ——— *Hypoleucus*, } ad Volgam admodum frequentes aves.

41. CHARADRIUS *Oedicnemus* in campis deſertis trans Volgam.

42. FULICA *atra*, fronte calva coccinea, corpore nigro virideſcente, pedibus lobatis viridibus; in paludoſis hiemat ſub aqua ut hirundines, quod ipſe in Boruſſia expertus ſum.

43. RALLUS *Crex* in humidis ad Volgam.

44. OTIS *Tarda* habitat inter ſata.

45. — *Tetrax* in campis deſertis trans Volgam obvia.

GALLINAE.

46. PAVO *criſtatus* a magnatibus alitur, & ova parit quae a gallinis & meleagridibus excluduntur.

47. Meleagris *Gallopavus* domesticus ubique alitur.
48. Phasianus *Gallus*. Copiose alitur haec species.
49. Tetrao *Tetrix* in betuletis cis Volgam.
50. ——— *Lagopus* hieme albus, exceptis rectricibus, & aliquot pectoris maculis rufis.
51. ——— *Coturnix* copiosa, in satis ex utraque parte Volgae.

Passeres.

52. Columba *oenas domestica*, in villis alitur.
53. ——— *Palumbus* in sylvis frequens.
54. ——— *Turtur* major domestica, colore griseo-incarnato, macula laterali colli nigra, in sylvis cis Volgam frequens.
55. Alauda *arvensis* in utraque Volgae parte, in campis admodum frequens.
56. ——— *Yeltoniensis rostro* cylindrico, subulato, recto; *lingua* bifida. *Unguis* posticus rectior; digito multo longior. *Rostrum* ad basin nigrum, crassiusculum, apice albescens. *Corpore* alisque nigris, in capite, dorso, & in summis alis, pennis paucis rufescentibus, sexta remigum margine exteriore alba; duae mediae rectrices rufae. Gregatim trans Volgam in vicinia lacus Yeltoniensis degunt hae Alaudae. *Alaudae Yeltonienses* appellantur. Saporis exquisiti, & admodum sunt pingues mense Augusto, magnitudine Sturni.
57. Sturnus *vulgaris*. In pratis trans Volgam innumeri sturnorum greges victitant.
58. Turdus *musicus* in sylvis ad Volgam.
59. Loxia *pyrrhula* in sylvis ad Volgam.

60. Em-

60. Emberiza *calandra*
61. ———— *citrinella* } in campis frequens.
62. Fringilla *coelebs* in ſylvis.
63. ———— *Carduelis*,
64. ———— *Linarius*, } in campis obviae.
65. ———— *Schoeniclus* in arundinetis ad Ylavlam & trans Volgam.
66. ———— *domeſtica* ubique frequens.
67. Motacilla *Luſcinia*; ſylvas cantu replent innumerae.
68. ———— *Curruca* in fruticetis ad Volgam.
69. ———— *flava* in campis reperitur.
70. ———— *alba*, pectore nigro, ad aquas frequens.
71. Parus *major* in ſylvis.
72. ———— *paluſtris* in ſalictis & arundinetis prope Volgam.
73. Hirundo *urbica*, ſub tectis nidulatur.
74. ———— *riparia*, in deſerto ad flumina Yerooſlan, Targun, Cuba, *etc.* in foraminibus victitat adeo frequens, ut multa millia ſimul videri poſſint.

CLASSIS TERTIA. AMPHIBIA.

Reptilia.

1. Testudo *lutaria*; minorem ovo excluſam at mortuam reperi, trans Volgam; innumerae hujus generis in lacubus ad Volgam habitare dicuntur.
2. Lacerta *agilis* in campis aeſtu torrentibus ubique obvia, griſea maculis nigris.

3. Rana

3. RANA *temporaria*, | ingens ranarum copia in paludibus ad Volgam; & quod obſervandum, jam hic clamoribus ſuis, aërem replent, cum in reliqua ſeptentrionali Ruſſia ſint mutae.
4. ——— *eſculenta*,

SERPENTES.

5. COLUBER magnus ad 2 & 5 pedes Angl. coloris griſei caeruleſcentis, maculis nigris, ſub abdomine luteſcens, venenoſus eſſe dicebatur; nonniſi partem exuviarum vidi.

6. ——— coloris lividi, caput triquetrum, corpore latiore macula coloris rubicundi in capite, abdomen craſſius; venenatus, in terra & aqua vivit, ſibilat; ad Volgam & Yerooſlanum invenitur: ad 3 pedes longus.

7. COLUBER coloris griſeo-fuſci, faſcia nigra teſſellata in dorſo, ſeſquipedem longus, in paludoſis vivit.

NANTES.

ACIPENSER. Spiracula lateralia ſolitaria, linearia.
Os ſub capite retractile edentulum.
Cirri ſub roſtro ante os, in omnibus quos vidi, quatuor.
Cauda ſurſum recurva.

8. ——— *Sturio* roſtro brevi obtuſo recto, cirrhi quatuor ante os.
Caput compreſſum, oſſa figuris aſteriſcorum inſcripta.

Corpus quinquangulare, pyramidatum, dorſo faſtigiato. *Cutis* ſcabra cinereo-flaveſcens. *Caro* rubeſcens.

Squamarum oſſearum ſeries quinque.

Series 1. *Dorſalis* ſquamis majoribus retrorſum uncinatis 14.

—— 2 & 3 *Laterales* 37.

—— 4 et 5 *Abdominales* inter pinnas pectorales & ventrales 11.

Longitudo 6 ad 16 pedes Angl.

9. ———— *Shyp*; *roſtro* elongato, acuminato, recto. *Cirri* 4 ante os, carne minus ſapido, a me non viſus.

10. ———— *Ruthenus major roſtro* elongato acuminato, veluti vagina tecto, paullulum ſupino. *Cirri* 4 ante os. *Caput* compreſſum, oſſa figuris aſteriſcorum inſcripta. *Corpus* quinquangulare, pyramidatum, dorſo faſtigiato. *Cutis* ſcabra flaveſcens, *Caro* rubra. *Squamarum* aſſearum ſeries 5. Magnitudo a 4 ad 10 pedes.

11. ———— *Ruthenus minor*, *roſtro* elongato acuminato recto. *Cirri* 4 ante os. *Caput* compreſſum 5. *Corpus* quinquangulare. *Cutis* ſcabriuſcula, atrocinerea. *Caro* rubra. *Squamarum* oſſearum ſeries 5. Magnitudo ad 4 pedes.

12. ———— *Ruthenus minor*, *roſtro* brevi obtuſo, recto. *Cirri* 4 ante os. *Caput* compreſſum, oſſa figuris aſteriſmorum inſcripta. *Corpus* quinquangulare, pyramidatum, dorſo faſtigiato. *Cutis* ſcabriuſcula, colore atro-cinereo, in abdomine flaveſcens, circa caudam teſſellata. *Caro* alba. *Squamarum* ordines 5 oſſearum, uncis parvis inſtructae.

Series 1 *Dorſalis* ſquamis majoribus retrorſum uncinatis 12, 13, 14 vel 15.

—— 2 & 3 *Laterales* 69 vel 70.

—— 4 & 5 *Abdominales* 10 vel 11 inter Pect. & Vent. pinnas.

Longitudo raro 3 vel 4 pedes excedit.

Pinna dorſalis prope caudam oſſiculis 46. Pin. Pect. 36. cum prima rigida. Pin. Ventr. 24. Anal. 27. Caud. 150.

13. ———— *Beluga* ſ. *Albula*. Roſtro brevi obtuſo recto. *Cirri* 4 ante os. *Caput* triquetrum. *Corpus* teres conicum. *Cutis* laevis, in dorſo nigricans, ſub abdomine alba, circa caudam teſſellata. *Caro* alba. *Squamarum* ſeries 5 depreſſae. *Longitudo* a 4 ped. Angl. ad 16. Tres ejus varietates dicuntur, quas non vidi.

Obſervatio. Omnes hi piſces ex membrana interiori (Angl. *ſound* dicta) dant Ichthyocollam, at Beluga praebet optimam. Sic & garum ex omnium acipenſerum ovis fieri poteſt, ex Rutheno minore optimum. Verno tempore raro hi piſces capiuntur, aeſtate pauci, ut plurimum tamen Rutheni minores. Autumno jam frequentiores capturae; hieme maximae piſcaturae ſub glacie inſtituuntur. Hi enim piſces in profundiſſima loca fluminum Volgae & Yaiki gregatim coeunt, & omnem locum ab imo fundo fluminis ad glaciem uſque replent. Hibernis copiis acipenſerum inventis, die poſt Feſtum Chriſti nati, omnes accolae ſecuribus & uncis conto longiſſimo impactis, inſtructi ad hunc locum contendunt; aperta glacie quilibet uncum demittit, & ope ejuſdem piſces in glaciem extrahit; ſi majoris molis piſcem eſt nactus, advocat

advocat vicinos ut opem ferant: junctis itaque viribus piscem extrahunt, & in commune vendunt. Sic quotannis multa millia Acipenserum capiuntur, & excepto Rutheno majore, omnes Caspii littoribusque vadosis ova deponere videntur; hujus solius pisciculi minuti duarum unciarum in fluminibus capiuntur, reliqui nunquam 8 unciis minores.

CLASSIS QUARTA. PISCES.

THORACICI.

1. PERCA *fluviatilis*, pinnis dorsalibus distinctis, secunda radiis sedecim, lineis utrimque sex nigris, pinnis ventralibus ruberrimis. Perch.
2. ——— *Lucio-perca*, pinnis dorsalibus distinctis, secunda radiis 25.
   Pinna Dorsi I. 14. II. 25. P P. 15. P. V. 6. P. A. 15. C. 22. cauda sinuata.
3. ——— *Lucio-perca minor*, pinnis dorsalibus distinctis, secunda radiis 22.

ABDOMINALES.

4. Esox *Lucius* Membr. Branchiost.
   P. D. 22. P. P. 14. P. V. 11. P. A. 18. C. 25.
5. CLUPEA Membr. Branchiost. VIII.
   P. D. 15. P. P. 14. P. V. 9. P. A. 19. C. 25.
   Latitudo ad Longitudinem $\frac{1}{7}$. Venter cultratus, abdominis carina spinis acutissimis praedita, squamae argenteae; saepe ad duos pedes longus.

6. CYPRINUS *Carassius*, cauda integra, squamis aureis nigricantibus, linea laterali recta.
P. D. 19. P. P. 15. P. V. 9. P. A. 8. C. 24.

7. ——— *Tinca*, cauda integra, corpore mucoso fusco.
P. D. 11. P. P. 17. P. V. 11. P. A. 10. C. 25.

8. CYPRINUS, an *Cephalus*? Dorso capiteque crasso, pinnis ventrali & anali rubicundis, squamis magnis; longus $\frac{1}{2}$ ped. usque ad 2 ped. Parisinos; latus 4 uncias, circa pinnas ventrales, cauda integra.

9. ——— *Idus*. Cauda bifida.
P. D. 11. P. P. 16. P. V. 9. P. A. 12. C. 23.

10. ——— *Jeses* pinnae ani & ventrales rubrae pectorales, fuscae, cauda bifida.
P. D. 11. P. P. 15. P. V. 9. P. A. 12. C. 22.

11. ——— an *Aspius*? capite acuto, squamis magnis, longus 2 ped. Parisin. lat uncias $5\frac{1}{2}$ circa pinnas ventrales.
P. D. 11. P. P. 18. P. V. 9. P. A. 16. C. 23.

12. ——— *Alburnus*.
P. D. 10. P. P. 16. P. V. 8. P. A. 18. C. 22.

13. ——— *Brama minor* pinnis omnibus nigricantibus, nonnisi magnitudine a Brama differt, nec tamen idem, aetate diversus.
P. D. 12. P. P. 15. P. V. 9. P. A. 24. C. 27.

14. ——— *Brama major*.
P. D. 12. P. P. 15. V. 9. A. 27. C. 28.

15. ——— *cultratus*, ventre acuto cultrato, dorso recto, squamis argenteis minoribus, longus usque ad duos pedes Parisinos, lat. uncias 4 $\frac{1}{2}$.
P. D. 10. P. P. 17. P. V. 8. P. A. $\frac{27}{28}$. C. 26.

16. CY-

16. Cyprinus, an *Ballerus?* admodum latus & tenuis, argenteis ſquamis.
P. D. 11. P. P. 16. P. V. 9. P. A. 29–30. C. 25.

Hic ſubſiſto; & Specimen Hiſtoriae Naturalis Volgenſis finio, ſperans fore ut eo & plures excitentur ad his perfectiora edenda.

Dabam Londini, die xxviii
Febr. anni 1767.

Joannes Reinholdus Forſter.

XXXIV. *De Problemate quodam Algebraico, deque evolutione mechanicæ cujuſdam Curvæ inter infinitas hypermechanicas, quæ determinatæ æquationi ſatisfaciunt. Auctore* Pio Fantoni, *Mathematico* Bononienſi. *Communicated by Sir* Horace Mann, *His Majeſty's Envoy at* Florence.

Read June 25, 1767.

QUI in computationibus analyticis verſari ſolet, animum non modo in ea præſidia ſolet intendere, quibus problemata deducuntur ad æquationes, ſed maximam ubique exoptat concinnitatem, atque elegantiam, ut univerſi operis apparatus, conſtructio, utilitaſque commendentur. Quantum vero elaborationis, ac ſtudii plerumque ad hæc ſingula requiratur, ii probe intelligunt excellentiſſimi viri, qui ſe jamdudum algebræ dediderunt. Veruntamen fateri ultro debent, non mediocrem aliquando utilitatem obtineri poſſe ex hujuſce potius, quam illius methodi applicatione. Fit enim non raro, ut cum quæſtionem aliquam ſubtili licet ingenio verſatus fueris, alia tandem methodus meliori auſpicio ſuſcepta, illico tibi elargiatur clariorem uberioremque ejuſdem quæſtionis ſolutionem, ex qua multa præterea obtineas quæ admireris. Id vero mihi

in ſublimi problemate quodam algebraico, an bene feliciterque contigerit, veſtro judicio, Academici ſapientiſſimi, quod maximi facio, decernendum relinquo. Si interea, ut exoro, hanc meam elucubrationem ſumma humanitate veſtra excipietis, eâ deinceps utar in aliis non contemnendis rebus tum phyſicis, tum mechanicis, ut veſtra ſapientia duce, facilius ad quaſdam naturæ adhuc reconditas leges pervenire poſſim.

Nunc porro velim intelligatis, me in hoc argumento analytico, de quo loquor, tria maxime præſtitiſſe. Primum enim curvam quandam exploravimus ſuis coordinatis ad axem, & licet lectiſſimis præſidiis uſi fuerimus in ſeparatione indeterminatarum, licet inveſtigationem noſtram ſatis ultro promotam conſpexerimus, in æquationem tandem, ut dicunt, hypermechanicam irrumpere opus fuit, in eam videlicet, quæ expoſcit mechanicam quadraturam curvarum exigentium primo mechanicam circuli quadraturam. Ex hac autem fere inextricabili conſtructione non ea certe conſectaria, quæ in votis erant, erui elegantiſſime potuiſſent. Quare difficultate rei veluti commoti, ſatis opportune curvam noſtram ab axe ad focum deduximus, atque hoc modo univerſum illud opus, cujus dilucide enodandi ſpem omnem antea demiſimus, eò tandem feliciter perduximus, ut ipſum recte eſſet, nobiſque plene ſatisfaceret. At vero in hac ſecunda problematis mei parte dum contendo, dum illam conſtanti animo defugio hypermechanicam conſtructionem, atque ad pure mechanicam propero, invenio tandem in infinita curvarum hypermechanicarum familia, quæ peculiari

cuidam

cuidam æquationi ſatisfaciunt, unam præterea curvam opportune abſcondi, quæ a ſimplici quadratura circuli dependeat, quæque nobis conſtructionem plenam aptioremque impertiat, ſitque ſemper in poteſtate, dummodo y dentur per x, quanquam ſeparari indeterminatæ nullo pacto ab invicem poſſint. Qui rem hanc altius perſcrutari curabunt, non difficili labore intelligent in æquationibus naturæ hujus ſemper includi curvam ſimilem noſtræ ſimili modo detegendam. Cujus ſane methodi cognitio an utilitati & commodo Analyſtis futura ſit in hujuſmodi operoſiſſimis quæſtionibus ſolvendis, non eſt cur diuiturniori oratione vobis exponam. Properamus itaque ad rem noſtram.

PROBLEMA.

TAB. XIV. FIGURA PRIMA.

Invenire curvam IMm ea proprietate donatam, ut ex dato puncto A ductâ in tangentem MT perpendiculari AG, intercepta MG ſit ſemper æqualis conſtanti a.

Ex puncto contactus M in axem AP duc perpendicularem MP, eique parallelam infinite proximam mp, tum excita ex puncto M rectam Ms parallelam Pp, dicque $AP = x$, $Pp = Ms = dx$, $PM = y$, $ms = dy$, & $MG = a$. Erit $Mm = \sqrt{dx^2 + dy^2}$. $PT = \frac{ydx}{dy}$. $TA = \frac{ydx - xdy}{dy}$, $TM = \frac{y}{dy}\sqrt{dx^2 + dy^2}$, ideoque $GT = \frac{y\sqrt{dx^2 + dy^2}}{dy} - a$.

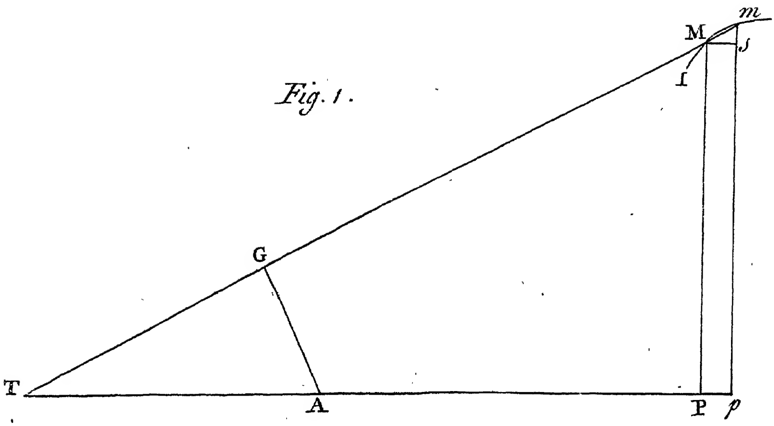


Fig. 1.

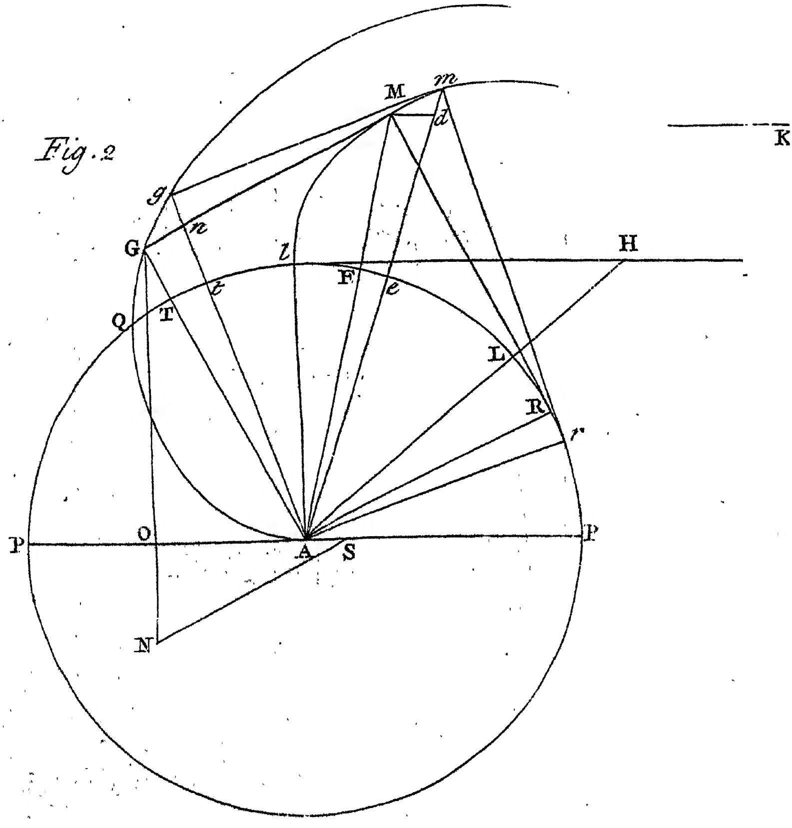


Fig. 2

Fig. 3.

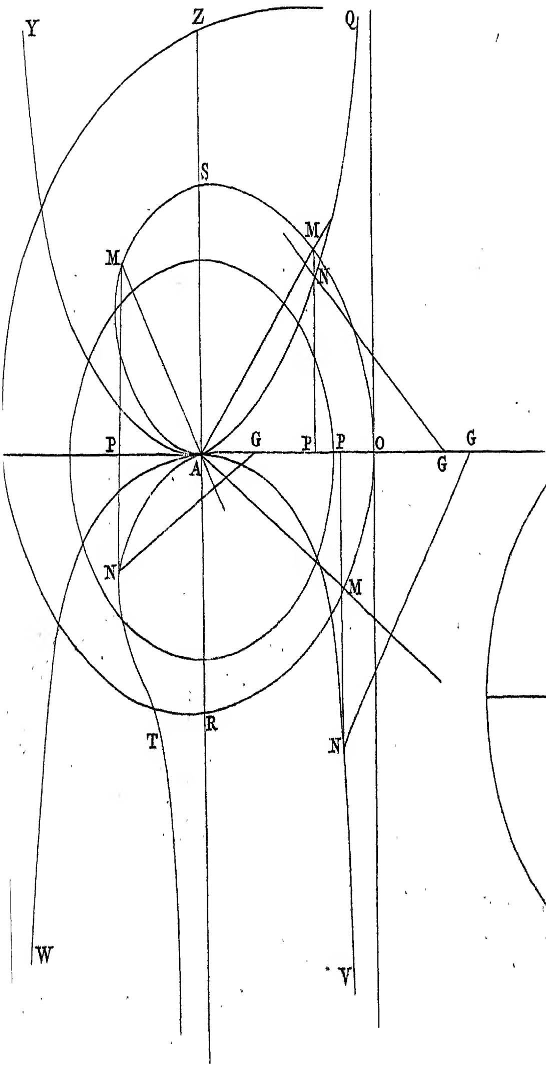
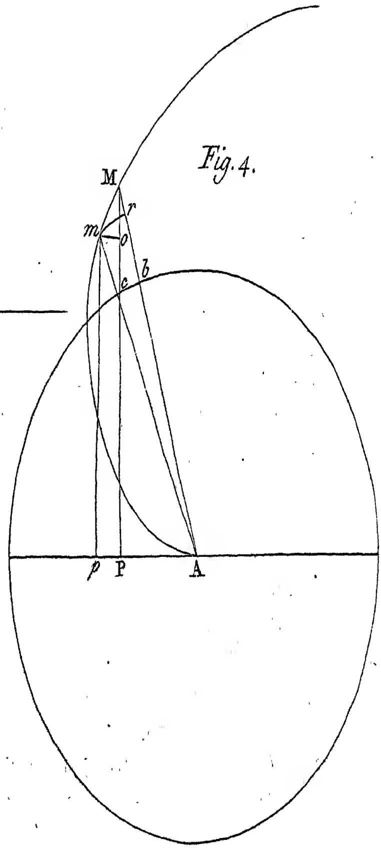


Fig. 4.



At propter similia triangula Msm, TGA, erit $Mm : Ms :: TA : TG$. seu $\sqrt{dx^2+dy^2} : dx :: \frac{ydx-xdy}{dy} : \frac{y\sqrt{dx^2+ay^2}}{dy} - a$; unde facta extremorum mediorumque multiplicatione, erit $a\sqrt{dx^2+dy^2} = ydy + xdx$, seu $x = -\frac{ydy}{dx} + \frac{a\sqrt{dx^2+dy^2}}{dx}$.

Fac autem pro separatione indeterminatarum $x = \int\frac{tdy}{a}$, & consequenter $dx = \frac{tdy}{a}$, tumque hos valores pro x, & dx substitue in data æquatione. Habebis $\int\frac{tdy}{a} = -\frac{ay}{t} + \frac{a\sqrt{t^2+a^2}}{t}$, & facta differentiatione, erit $\frac{tdy}{a} + \frac{ady}{t} - \frac{aydt}{tt} = \frac{-a^3dt}{t^2.\overline{a^2+t^2}|^{\frac{1}{2}}}$ seu $\frac{dy}{y} - \frac{a^2dt}{t.\overline{a^2+t^2}} = \frac{-a^4dt}{ty.\overline{a^2+t^2}|^{\frac{3}{2}}}$.

Cum porro ex nota Bernoulii methodo sit $\frac{a^2dt}{t.\overline{a^2+t^2}} = \frac{dt}{t} - \frac{tdt}{t^2+a^2}$, pone claritatis gratia hasce logarithmicas quantitates $= \frac{dn}{n}$. Hinc habebis $\frac{dy}{y} - \frac{dn}{n} = \frac{-a^4dt}{ty.\overline{a^2+t^2}|^{\frac{3}{2}}}$. Positis autem $\frac{dy}{y} - \frac{dn}{n} = \frac{dp}{p}$, atque ideo $\frac{y}{n} = \frac{p}{a}$, seu $y = \frac{np}{a}$, & facta harum quantitatum substitutione, obtinebis $\frac{dp}{p} = \frac{-a^5dt}{npt.\overline{a^2+t^2}|^{\frac{3}{2}}}$, seu $dp = \frac{-a^4dt}{t^2\times\overline{t^2+a^2}}$; posito videlicet jam primum $n = \frac{at}{\sqrt{t^2+a^2}}$ ex quo sequitur $dp = -\frac{a^2dt}{t^2} + \frac{a^2dt}{a^2+t^2}$.

 Sed

Sed quia constituimus superius $x = \int \frac{t\,dy}{a}$, & $y = \frac{np}{a}$; si in hisce valoribus coordinatarum x & y substituas æquivalentes valores expressos dumtaxat per t, & dt, habebis demum

$$x = \int \overline{\frac{a\,t\,dt}{\overline{t^2 + a^2}|^{\frac{3}{2}}} \times \int \frac{a^2 dt}{t^2 + a^2}}$$

$$\& \; y = \frac{t}{\overline{a^2 + t^2}|^{\frac{1}{2}}} \times \overline{\frac{a^2}{t} + \int \frac{a^2 dt}{t^2 + a^2}}.$$

Qui vero hujusmodi formulas ad constructionem revocare statuerit, intelliget ille quidem infinitis dumtaxat curvis problema nostrum plane exhauriri posse. *At quis non dixerit $\int \overline{\frac{a\,t\,dt}{\overline{t^2 + a^2}|^{\frac{3}{2}}} \times \int \frac{a^2 dt}{t^2 + a^2}}$, exigere constanti lege in quolibet casu quadraturam mechanicæ cujusdam curvæ, quæ ipsa primum a mechanica circuli quadratura dependeat? Primo certe hujusce formulæ adspectu nemo non judicaverit problema nostrum hypermechanicum fore; maxime vero cum nulla directa methodo, quantum mihi constat, compertum sit, hujusmodi formulas revocari posse ad alias, quæ a sola circuli quadratura dependeant. Quapropter in illa ego opinione adhuc essem, ut solæ hypermechanicæ curvæ aptæ forent satisfaciendo problemati nostro, si quæsitam curvam ab axe non traduxissem ad focum; ex quo illico certior factus sum, quæstiones hujusmodi,

* Fluens $\int \overline{\frac{at\,dt}{\overline{t^2 + a^2}|^{\frac{3}{2}}} \times \int \frac{a^2\,dt}{t^2 + a^2}}$ a sola circuli quadratura pendet & hæc est $\frac{at}{\overline{a^2 + t^2}|^{\frac{1}{2}}} - \frac{a}{\overline{a^2 + t^2}|^{\frac{1}{2}}} \times \int \frac{a^2\,dt}{a^2 + t^2}$ (arc circuli, cujus radius est a & tangent t.) E. W.

quas

quas ab initio dixeris implicatiſſimas, ſeu pene inextricabiles, ſola tandem circuli quadratura expediri feliciter poſſe. Quo autem modo id factum a nobis fuerit brevi expono.

Figura Secunda.

Referatur * quæſita curva IMm ad focum A, ex quo ductis duabus ordinatis AM, Am minimum angulum continentibus, centro A, radio AM deſcribatur infiniteſimus arcus Md, tum vocetur AM $= z$, M$d = dx$, M$m = ds$; ductaque tangente MG, atque in ipſam ex puncto A perpendiculari AG, fiat intercepta MG $= a$, unde perpendicularis AG erit $= \sqrt{z^2 - a^2}$.

Propter ſimilia triangula AMG, Mdm erit Mm : md : : MA : MG; ſeu $ds : dz : : z : a$. ergo $zdz = ads$. & integrando $Aa + as = \frac{z^2}{2}$. Conſtat itaque curvam quæſitam eſſe rectificabilem, eſtque A quantitas addenda, ſi opus fuerit, æquationi complendæ, quam A deinceps determinabimus.

Erigatur interea æquatio differentialis ad quadratum, & orietur $z^2dz^2 = a^2ds^2 = a^2dz^2 + a^2dx^2$, ob triangulum M$dm$ infiniteſimum rectangulum in d. Hanc ergo habebis $\overline{z^2 - a^2}.dz^2 = a^2dx^2$; ſive $dz\sqrt{z^2 - a^2} = adx$, quæ eſt æquatio quæſitæ curvæ relatæ hoc modo ad focum A.

Multiplicetur hæc ultima æquatio per $\frac{z}{2a}$; fiet $\frac{zdz}{2a}\sqrt{zz - aa} = \frac{zdx}{2}$. Integretur; habebis aB +

* Hujus curvæ arcum, longitudinem, evolutam & radium curvaturæ jamdudum invenit Simpſon; conſulas enim pagin. 151 & 163 in tractatu ſuo de fluxionibus. E. W.

$\frac{\overline{zz - aa}\sqrt{zz - aa}}{2.3.a} = \int \frac{zdx}{2}$. Cum autem $\frac{zdx}{2}$ ſit elementum areæ, patens eſt curvam eſſe quadrabilem. B eſt quantitas addenda, ſi opus ea fuerit, in integratione.

Ut vero redigatur æquatio ſuperius inventa ad arcum radii conſtantis, abſcinde AE $= a$, & deſcribe arcum minimum Ee, quem voca $= du$. Habebis $z : a :: dx : du$. ergo $dx = \frac{zdu}{a}$; quo valore ſubſtituto in ſuperiori æquatione $dz\sqrt{z^2 - a^2} = adx$, hæc mutabitur in iſtam $\frac{dz}{z}\sqrt{z^2 - a^2} = du$, in qua inſunt variabiles ſeparatæ.

Ut primum membrum ad formulas magis cognitas reducatur, ita æquationem diſpono $\frac{zdz\sqrt{z^2 - a^2}}{zz} = du$; tum conſtituo AG $= \sqrt{z^2 - a^2} = t$, factaque ſubſtitutione, orietur $\frac{t^2dt}{t^2 + a^2} = dt - \frac{a^2dt}{t^2 + a^2} = du$. Formula $\frac{a^2dt}{t^2 + a^2}$ ut conſtat, eſt elementum arcus circularis, cujus radius $= a$, tangens $= t$.

Ultima igitur hæc æquatio ad conſtructionem perducit, quæ circuli quadraturam ſupponit. Centro itaque A, radio AI $= a$, deſcribatur circulus ILP; cui ſit tangens indefinita IK. Sumatur in hac tangente quælibet IH $= t$, & agatur ſecans AH $= z$; ſume præterea differentiam inter tangentem IH, & ejus arcum IL, quæ erit $= u$; tandem accipe arcum IL huic differentiæ æqualem, & per punctum E duc AM = AH, punctum M erit in curva quæſita.

Ex

Ex hac conſtructione facile colligitur curvam noſtram incipere in puncto I, tum ad modum ſpiralis ſemper recedere a circulo, & infinitis circumvolutionibus illum ambire. In puncto I curva tangitur a radio IA. Nullam addidi in mea conſtruſtione conſtantem, propterea quod conſtantis additio curvam non mutat. Nam IE vel ſit æqualis u, vel $u+b$, vel tandem $u-b$, eadem prorſus curva enaſcitur.

Nunc vero ſunt determinandæ conſtantes A & B, quæ additæ ſunt in integratione, dum curvæ rectificationem, & quadraturam invenimus. Quoniam poſito $s=o$, fit $z=a$, æquatio $\frac{z^2}{2}-Aa=as$, data hac hypotheſi, in iſtam mutabitur $\frac{aa}{2}-Aa=o$, unde $A=\frac{a}{2}$; quapropter æquatio completa erit $\frac{zz-aa}{2a}=s$. Atqui $zz-aa=H$. ergo $\frac{H}{2a}=s$.

Quod ſpectat ad quadraturam, jam conſtat fore aream AIM $=o$, cum fit $z=$ radio, ſeu $=a$; ergo æquatio $aB+\frac{\overline{zz-aa}\sqrt{zz-aa}}{2.3.a}=\int\frac{zdx}{2}$, evadit in hac hypotheſi in iſtam $aB=o$: ergo æquatio completa eſt $\frac{\overline{zz-aa}\sqrt{zz-aa}}{2.3.a}=\frac{t^3}{2.3.a}=\int\frac{zdx}{2}$. Sed $\frac{t^2}{2a}=s$; ergo $\frac{ts}{3}=\int\frac{zdx}{2}$; ideoque ſpatium IAM eſt tertia pars rectanguli ex AG, ſeu IH, & ex curva IM.

Radium oſculi hac ratione definiemus. Ducatur radius AR perpendicularis rectæ AG, & jungatur RM. Quoniam GM, AR æquales ſunt, & parallelæ, GA, MR pariter æquales erunt, & parallelæ. Ergo MR

MR perpendicularis radio AR tanget circulum, & perpendiculariter occurret curvæ Mm. Eodem prorſus modo ducto radio Ar normali rectæ Ag, linea mr erit tangens circuli, & normalis curvæ Mm. Igitur curva IMm ea eſt quæ naſcitur ex evolutione circuli, & recta MR $=$ AG æquabit arcum circularem IR.

Quoniam vero RM$=$AG$=$IH, & IH ex conſtructione æquat duos arcus circulares IE, IL, arcus IR æquabit duos arcus IE, IL, & dempto communi IL, remanebit arcus IE $=$ LR.

Infiniteſimus ſector RMm, qui eſt elementum areæ REIM æqualis eſt $\frac{tds}{2}$. Sed $ds=\frac{zdz}{a}=\frac{tdt}{a}$. ergo R$Mm=\frac{t^2dt}{2a}$. Et integrando area REIM$=\frac{t^3}{2.3.a}$. Sed etiam area IAM ſuprà inventa eſt æqualis $\frac{t^3}{2.3.a}$, ergo area REIM $=$ IAM. Et ablato ſpatio communi IEM, remanet ſector IAE$=$MER. Addito autem ſectore EAR fit ſector IAR $=$ triangulo AMR, quod apprime cum veritate conſentit; nam cum arcus IR $=$ RM, conſtat ſectorem IAR æquare triangulum ARM.

Curva tranſiens per omnia puncta G.g. erit baſis, ex qua gignitur tractoria IMm. Quænam ſit hæc curva breviter videamus. Quoniam GAn, & MRm ſunt ſectores ſimiles, & AG $=$ RM, erit G$n=$ Mm $=ds$. Ergo æquatio $ads=tdt$, erit æquatio curvæ quæſitæ, exiſtente ordinata AG$=t$, G$n=ds$. Ut autem æquatio reducatur ad arcum radii conſtantis, vocetur T$t=dw$; erit $t:a::ds:dw$. Ergo $ads=tdw$.

Ergo

Ergo $tdt = tdw$, five $dw = dt$, five tandem $Tt = gn$, quæ eſt æquatio ſpiralis Archimedeæ, cujus conſtructio ita peragitur.

Age radium AP, perpendicularem radio AI, & ſumatur arcus PQ æqualis radio; tum polo A, deſcribatur ſpiralis Achimedea tranſiens per punctum Q; hæc ipſa erit baſis, ex qua deſcribitur tractoria IM prædita tangente conſtanti GM $= a$.

Interea hæc habe: ſpiralis Archimedea eſt ea curva, a qua tamquam baſi noſtra generatur tractoria IMm. Nunc ſupereſt animadvertere, quod ſi in illa formula, quam vir clariſſ. Vincentius Riccatus methodo motus tractorii conſtruxit in ſuo commentario de uſu hujus motus in æquationum differentialium conſtructione (ubi hanc methodum illuſtravit penituſque abſolvit) ſi, inquam, in illa formula ſupponas x & y eſſe coordinatas ſpiralis Archimedeæ, & y datas eſſe per x, quamquam indeterminatæ ſeparari omnino nequeant, ſuſcipiet dicta formula ex infinitis, quarum eſt capax, unam quoque conſtructionem dependenter a noſtra curva. Ea ex quatuor Riccatianis ibidem expoſitis formulis, quæ hypotheſi noſtræ convenit prima eſt, nimirum,

$$\frac{abdz}{\sqrt{bb+qq}} + qdx = bdy.$$

Facta ergo, ut dixi, ſuppoſitione, ejus x & y eſſe coordinatas ſpiralis Archimedeæ, ſi infinita puncta N conſtruendæ curvæ tuto invenire cupias, exigit illa methodus, ut deſcripta tractoria IMm ope fili, ſeu tangentis conſtantis GM $= a$, facto jam motu aG verſus Q, tumque ſumpta in axe quacumque conſtanti OS $= b$, ſemper ad eandem partem, ſi per punctum S ducas

ducas parallelam tangenti GM, donec occurrat ordinatæ $OG=y$ in puncto N, hocce punctum, ut ibi demonstratur, est semper in quæsita curva. Atqui vidimus supra rectam RA parallelam tangenti GM hujus nostræ tractoriæ IM*m* fore perpendicularem radio AG spiralis Archimedeæ AQG.

FIGURA TERTIA.

Ergo ut habeas infinita puncta N.N. construendæ curvæ, sufficit quod sumas semper in axe constantes PG, $PG=b$ ad eandem plagam, tumque a punctis G, G ducas in radios spiralis AM, AM productos, si oporteat, normales GN, GN, donec occurrant ordinatis PM, PM in N.N. Hoc modo obtinebis per infinita puncta curvam hac methodo describendam. Invenies itaque hujusmodi curvæ ramum genitum a spiralis arcu AMS esse ANT; ab altero vero spiralis arcu SMO esse ANQ; a tertio OMR esse ANV; a quarto RK esse AY; a quinto denique KZ esse AW, & sic in infinitum assymptoticos omnes; ex quo propterea vides integram curvam, quæ nostræ formulæ constructionem suppeditat in hac videlicet peculiari tractoria abdita ramis numero infinitis gaudere, ac eorum quemlibet votis satisfacere recte posse.

Sed quia ad obtinendam dictæ formulæ constructionem opus maxime est ut abscissæ x sint in axe, earum vero ordinatæ y sint omnes inter se parallelæ (nostræ autem y hic sunt ad focum) ac propterea oportet ut eædem y datæ sint per x, vel postea separari indeterminatæ possint, vel non, nunc ergo ut hisce conditi-

onibus

onibus compleam, ſatis mihi erit invenire æquationem ſpiralis Archimedeæ relatæ ad axem, quod ſic aſſequor.

Figura Quarta.

Sit ſpiralis Archimedea A*m*M, ejus axis FAF, abſciſſa $AP = x$, ordinata PM ad angulum rectum $= y$, eique infinite proxima *pm*. Ducta *mo* parallela ad axem, erit, $mo = dx$, $oM = dy$. Sit propterea AM radius ſpiralis $= t$, cum quo A*m* faciat angulum infiniteſimum MA*m*, & centro A, radio AM, deſcripto circuli arcu *mr*, erit $Mr = dt$. Voca arcum $mr = ds$, & eodem centro A, radio quovis conſtanti $= a$, deſcribe circulum F*c b*, & voca ejus arcum infiniteſimum $cb = du$.

Ex hac præparatione erit primò $\overline{AM}^2 = \overline{AP}^2 + \overline{PM}^2$, ſeu $t^2 = x^2 + y^2$, & $t = \sqrt{x^2 + y^2}$; unde $dt = \frac{xdx + ydy}{\sqrt{x^2 + y^2}}$. Præterea habebis $\overline{Mr}^2 + \overline{rm}^2 = \overline{Mm}^2$ ſeu $dt^2 + ds^2 = dx^2 + dy^2$. Sed ex ſimilitudine Sectorum A*cb*, A*mr*, eſt $Ac : cb :: Am : mr$; ſeu $a : dt :: t : ds$. & ex æquatione ſpiralis Archimedeæ ad focum habes $cb = Mr$, ſeu $du = dt$; unde erit $ds = \frac{tdt}{a}$. Ergo factis opportune ſubſtitutionibus in altera ſuperiori æquatione, obtinebis $dt^2 + \frac{t^2dt^2}{a^2} = dx^2 + dy^2$, ſeu tandem

$\overline{a^2 + x^2 + y^2} \times \overline{\frac{xdx + ydy}{x^2 + y^2}} = a^2 \times \overline{dx^2 + dy^2}$. vel potius

 $dy^2 +$

$$\overline{dy}^2 + \overline{2dxdy} \times \frac{a^2xy + x^3y + xy^3}{x^2y^2 + y^4 - a^2x^2} + \overline{dx}^2 \times \frac{x^4 + x^2y^2 - a^2y^2}{x^2y^2 + y^4 - a^2x^2} = 0$$

unde completo quadrato, & facta radicis extractione, erit

$$\frac{dy}{dx} = \frac{-\overline{xy} \times \overline{a^2 + x^2 + y^2} \pm a \times \overline{x^2 + y^2}^{\frac{3}{2}}}{x^2y^2 + y^4 - a^2x^2}.$$

Ecce itaque ſpiralis Archimedeæ æquationem relatæ ad axem, ut optabamus, in qua y datur per x. * Quamquam vero in hujuſmodi æquatione, indeternatæ ſeparari nullo artificio poſſint, vides tamen præſidiis pure mechanicis ad conſtructionem nos feliciter perveniſſe, quod, attento illius ſummatoriæ adſpectu, quam initio obtinuimus, cum curvam noſtram ad axem referre placuit, impoſſibile videbatur.

Scio ego quidem conſtructionem hancce, quæ a ſola circuli quadratura dependet, non penitus exhaurire ſupradictam Riccatianam formulam, quippe quæ conſtrui etiam poteſt, quacumque alia propoſita tractoria, cujus baſis ſit dicta ſpiralis Archimedea, ejuſque tangens recta quævis linea conſtans; ſed inter infinitas haſce conſtructiones noſtra quidem maximum locum habet, ut quæ cæteris ſimplicior, nec minus vera.

Porro antequam finem facio, unum addam. Laudatus Mathematicus in capite ſecundo ſui commentarii oſtendit, quod ubi in conſtructione ſuæ formulæ tractoriam circuli adhibeat, tunc in infinitas decurrit tranſcendentes curvas, quæ ſimul exhaurire valent

* Ad hunc modum indeterminatæ ſeparari poſſunt; ſubſtituatur pro $x = \frac{z}{a} \times \int \frac{a\,dz}{\sqrt{a^2 - z^2}}$, & pro $y = \frac{\sqrt{a^2 - z^2}}{a} \times \int \frac{a\,dz}{\sqrt{a^2 - z^2}}$, & ſit. E. W.

propoſitam.

propoſitam formulam. Sed (quod ei merito quidem in pretio eſt) in infinita harum curvarum familia unam inſuper latentem detegit algebraicam quarti gradus, quæ commodè ejuſdem formulæ conſtructionem ſuppeditat, rectèque perficit. Nos in re fortaſſe difficiliori non diſſimile exemplum hic attulimus. Vidimus enim problema noſtrum, quod per tractoriam ſpiralis Archimedeæ generatim conſtruitur, expoſcere & ipſum ad ſui conſtructionem curvas numero infinitas, ſed quod moleſtius videtur, magiſque operoſum, hujuſmodi eſſe haſce curvas, ut niſi hypermechanico labore poſſimus aſſequi. Veruntamen in infinito harum agmine facile & nobis fuit oſtendere unam præterea curvam abſcondi, quam illico aſſequaris dependenter a ſola quadratura circuli, ideoque attenta rei difficultate, multo ſimpliciori modo, quàm initio ſperare licuiſſet. Noverim certe curvam hanc noſtram non plene exhaurire datam formulam, ſed infici nequit, ejuſdem exhibere nullo fere negotio rectiſſimam, maximeque ſimplicem conſtructionem, quod ſatis eſt, aliiſque planiorem viam oſtendere, qua facilius enodare poſſint hujuſce generis quæſtiones inextricabiles primo intuitu, nec vero labore vacuas. Hoc itaque inventum credidimus non contemnendum fore, præſertim cum aliæ methodi uſque adeo notæ, quovis verſatæ ſtudio, minime quantum nobis conſtat, ad id commodum perducere valeant.

Romæ prid. Non. Aprilis, 1766.

Pius Fantonus,

Philoſophus & Mathematicus Bononienſis.

XXXV. *A Memoir concerning the most advantageous Construction of Water-Wheels,* etc. *by Mr.* Mallet *of* Geneva. *Communicated by* M. Maty, *M. D. Sec. R. S. Translated from the* French, *by* J. Bevis, *M. D. R. S. S. Read* March 26, 1767.

§ I. THE stream of rivers is of such importance in moving machines of all kinds, that any attempt towards perfecting this part of mechanics may be considered as of very great utility.

The first wheel on which the stream acts is one of the most essential members of the machine, and it is easy to discern that the greater or less effect thereof must depend, in a great measure, on the manner of constructing this wheel, and on the dimensions given to it. I shall not at present inquire whether wheels of different constructions from those which have been long in use might be advantageously substituted in their stead; but confine myself, in this essay, to an examination of the most common ones, and to discover the means by which they may be made to produce the greatest possible effects. Their construction is very simple; they consist of several planes inserted into the same axle placed horizontally above the surface of the water, and in a position perpendicular to the stream. These planes, called

float-

Fig. 1.

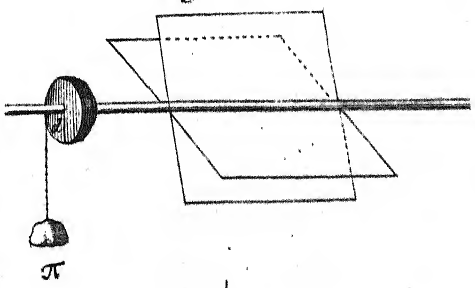


Fig. 2.

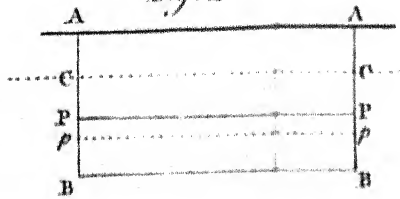


Fig. 3.

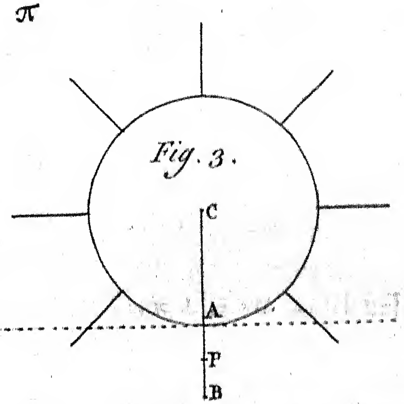


Fig. 4.

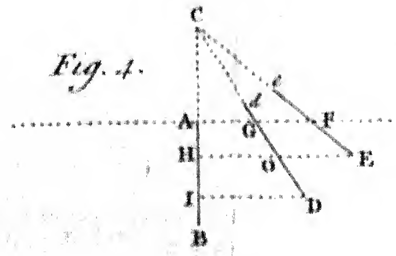


Fig. 5.

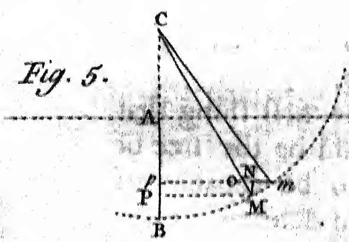


Fig. 6.

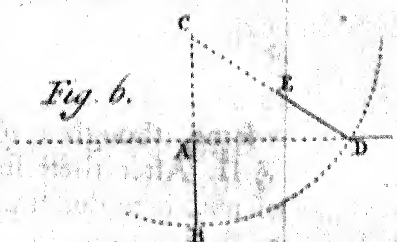
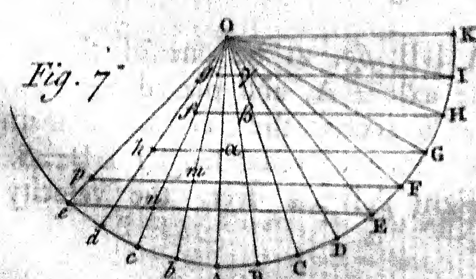


Fig. 7.



J. Mynde sc.

float-boards, by yielding to the action of the ſtream, cauſe the axle on which they are fixed to turn round, by means of ſeveral wheels, which take into each other, and give motion to the part deſtined to produce ſome purpoſed effect, as the mill-ſtone in a corn-mill.

The ſize of the float-boards, the velocity with which the wheel is to turn, and the number of the float-boards to produce the greateſt poſſible effect, are three main things I propoſe to examine in the following inquiry.

In the firſt place I will ſuppoſe the total reſiſtance which this wheel has to encounter, on the part of the machine, and which hinders it from moving ſo ſwift as the ſtream, to be expreſſed by a weight π, ſuſpended to the extremity of a cord fixed to the circumference of a wheel whoſe radius is d, and which has the ſame axle as the float-board wheel, ſo that the effect of the ſtream is to raiſe the ſaid weight π, as expreſſed in Tab. XV. *fig*. 1. I will likewiſe ſuppoſe, that the ſtream, by its velocity, moves through v feet in one ſecond of time, and that this velocity is the ſame, though at different depths.

§ II. After theſe ſuppoſitions, the firſt thing that preſents, is to determine what ſhould be the ſize of the float-boards for the ſtream to be capable of raiſing the weight π with a certain determinate velocity.

Let AA BB, *fig*. 2. be one of the float-bords let into the axle AA, and placed vertically in the water, ſo as to receive the perpendicular impulſe of the ſtream. Its horizontal length BB$=\beta$ feet, its vertical height AB$=\alpha$ feet, the velocity of the wheel

wheel at the point B, ſuch that it ſhall run through z feet in a ſecond; n pounds the weight of a cubic foot of water; and I will ſuppoſe the impulſe of the ſtream on a plane perpendicular to it is (as Dr. *Daniel Bernoulli* has ſtated in his *Hydrodynamica)* equal to the weight of a priſm of water, whoſe baſe is the plane, and its altitude the generating height of the velocity with which the plane is impelled. This being ſuppoſed; let AP$=x$, Pp, its differential, $=dx$, which will give the velocity of the float-board at the point $p=\frac{x}{a}z$, and the relative velocity of the ſtream with which the plane is impelled at the ſame point $= v - \frac{x}{a}z$ whoſe generating height is $\frac{(v-\frac{x}{a}z)^2}{60}$ feet, whence we have the weight of the parallelopiped of that height, and of the baſe PP pp equal to $\frac{n\beta dx}{60}(-v\frac{x}{a}z)^2$ pounds, which weight multiplied by the length AP (x) of the lever which tends to turn the plane, will give $\frac{n\beta xdx}{60}(v-\frac{x}{a}z)^2$ for the total effect of the ſtream on the little rectangle PP pp, whoſe integral is $\frac{n\beta}{60}\int.(vvxdx - \frac{2vz}{a}xxdx + \frac{zz}{aa}x^3dx) = \frac{n\beta}{60}(\frac{1}{2}vvxx - \frac{2}{3}vz\frac{x^3}{a} + \frac{1}{4}zz\frac{x^4}{aa} - \frac{1}{2}vvff + \frac{2}{3}vz.\frac{f^3}{a} - \frac{1}{4}\frac{zzf^4}{aa})$ (putting AC $=f$ for the diſtance between the axle and the ſurface of the water when the float-board has only its part CB plunged in the water) which (putting $x = a$) will become $\frac{n\beta}{60}(\frac{1}{2}vv - \frac{2}{3}vz + \frac{1}{4}zz.aa - \frac{1}{2}vvff + \frac{2}{3}\frac{vzf^3}{aa}$

 $-\frac{1}{4}$

$-\frac{1}{4}\frac{zzf^4}{\alpha\alpha})$ and will express the effect on the whole plane CCBB, equal π d, the product of the weight π by the length d of the lever on which it acts in opposing the motion of the wheel.

§ III. If the wheel be plunged as deep as its axle, that is, $f = o$, the equation is changed into this $\frac{n\beta\alpha\alpha}{60}(\frac{1}{4}vv - \frac{2}{3}vz + \frac{1}{4}zz) = d\pi$, where it appears 1°. That the quantities d, π, v and z remaining the same, we have β inversely proportional to the square of α, whence it follows, that if the length β is to be diminished without altering the effect of the float-board, the height α must be encreased proportionally to the square root of β; for example, if β is to be made four times less, it will be sufficient to double the height α. 2° That likewise the velocity of the float-board remaining the same, the weight π will be in the compound ratio of the length β, and of the square of the height $\alpha\alpha$. 3° Without meddling with the dimensions of the float-board, the more the quantity z is increased, the more must the weight π be diminished. If z be made $= o$, we have $\pi = \frac{n\beta\alpha\alpha}{60d}\cdot\frac{1}{4}vv$, and if $z = v$ we have $\pi = \frac{n\beta\alpha\alpha}{60d}\cdot\frac{1}{12}vv$, that is six times greater than in the first case; which is very conformable to the nature of things, for when the wheel is in motion, the stream then not acting upon it but with the excess of its velocity above that of the wheel, it follows, that the greater such velocity is, the more will the effect of the stream be diminished.

It

It follows from our laſt remark, that the greateſt weight with which the ſtream can conſtitute an equilibrium, will be $= \frac{n\beta\alpha\alpha vv}{120d}$, but then the wheel will not have any motion, nor conſequently the weight π: If the float-board be increaſed, or the weight diminiſhed, from that inſtant the wheel will begin to turn, and the ſwifter as the float-board is greater, or the weight leſs; but in moſt machines, it is required that the weight may be the greateſt poſſible, as alſo the velocity with which it is raiſed. A queſtion therefore here offers itſelf, whoſe ſolution is of much importance. What muſt be the velocity of the float-board whoſe dimenſions are given, that the product of the weight by its velocity ſhall be the greateſt poſſible?

§ IV. The velocity of the weight π is $\frac{d}{\alpha} z$ feet in a ſecond, which being multiplied by the value of $\pi = \frac{n\beta\alpha\alpha}{60d}\left(\frac{1}{2} vv - \frac{2}{3} vz + \frac{1}{4} zz\right)$ will give the product $\frac{n\beta\alpha}{60}\left(\frac{1}{2} v^2 z - \frac{2}{3} vzz + \frac{1}{4} z^3\right)$ which muſt be a *maximum*; for which purpoſe make $\frac{1}{2} vvdz - \frac{4}{3} vzdz + \frac{3}{4} z^2 dz = 0$; whence we have $z = \frac{8 - \sqrt{10}}{9} = 0{,}53752v$: this value of z being ſubſtituted, make the equation $\frac{1}{2} vv - \frac{2}{3} vz + \frac{1}{4} zz = \frac{11 + 2\sqrt{10}}{81} vv$, ſo that we have the equation $\beta\alpha\alpha = \frac{11 + 2\sqrt{10}}{4860} + \frac{d\pi}{nvv} = 280{,}529 + \frac{d\pi}{nvv}$, which expreſſes the dimenſions of the float-board where the effect will be the greateſt poſſible. If the float-board be plunged no deeper than to CC, as we have at firſt ſuppoſed, the moſt advantageous value of

of z may be determined in the fame manner, which will be found

$$z = \frac{8\alpha^4 - 8\alpha f^3 - \alpha\sqrt{10\alpha^6 + 54\alpha^4 f^2 - 128\alpha^3 f^3 + 54\alpha^2 f^4 + 10 f^6}}{9(\alpha^4 - f^4)} v$$

If $f = 0$, this value of $z = 0{,}537v$

$f = 0{,}25\,\alpha = \frac{1}{4}\alpha \quad = 0{,}498v$

$f = 0{,}3\,\alpha \quad = 0{,}486v$

$f = 0{,}5\,\alpha = \frac{1}{2}\alpha \quad = 0{,}436v$

$f = 0{,}7\,\alpha \quad = 0{,}390v$

$f = 0{,}9\,\alpha \quad = 0{,}353v$

$f = \alpha \quad = 0{,}333v$

By the infpection of thefe different values it appears that this value of z diminifhes as the plunged part is greater, and that this velocity can never exceed the quantity $0{,}537v$, nor be lefs than $\frac{1}{3}v$ *.

This value of z and of its fquare zz being fubftituted in the general formula (§ II.) we fhall obtain from it the following equation:

$$\frac{60\,d\,\pi}{u\beta vv} : \frac{-27\alpha^4 f^2 + 32\alpha f^3 - 27\alpha^2 f^4 + 11 f^6 + 2(\alpha^3 - \alpha ff)\sqrt{10\alpha^6 + 54\alpha^4 f^2 - 128\alpha^3 f^3 + 54\alpha^2 f^4 + 10 f^6}}{81(\alpha^4 - f^4)}$$

which for a given relation between f and α will fhew the breadth β for producing the greateft effect.

* If in the value of z we make $f = \alpha$, we have $z = \frac{0}{0}$ which obliges us to take, according to the common method, the differentials of the numerator and of the denominator, confidering f as variable, and the relation of thefe differentials will give the value of z: but on account of the radical quantity, the calculus being fomewhat tedious, and, again bringing out $z = \frac{0}{0}$ and that after feveral fimilar operations, it is better to have recourfe to the equation from which the value of z was deduced; this equation is $\frac{4}{3}\alpha v z \cdot \frac{\alpha^3 - f^3}{\alpha^4 - f^4} - \frac{1}{2}\alpha\alpha vv \cdot \frac{\alpha^2 - f^2}{\alpha^4 + + f^4}$, which by the above operation will be $\frac{3}{4}zz = vz - \frac{1}{4}vv$, and $z = \frac{1}{3}v$.

 As

As the extremity of the float-board muſt have a certain velocity depending on the relation of the height α to the plunged part, and as the velocity of the weight $\pi = \frac{d}{\alpha} z$, it follows that if we would increaſe the velocity of π, we muſt diminiſh the height α and increaſe the breadth β, ſo that the product $\beta\alpha\alpha$ and the relation of f to α may be the ſame as before; for example, if the wheel be plunged as deep as the axle, to double the velocity of the weight, the height of the float-board muſt be reduced one half, and its breadth be quadrupled.

§ V. It may ſo happen that the channel on which the wheel is placed ſhall be ſo ſhallow and narrow, as not to allow the float-boards the neceſſary dimenſions, for raiſing the weight with a convenient velocity. In this caſe we are obliged to raiſe the axle of the wheel above the ſurface of the water, ſo much that the lever on which the ſtream acts may be long enough to recompenſe the ſmallneſs of the float-boards. Herein it is neceſſary to ſolve the following problem:

The breadth b, *and the height* a, *of the float-board* AB *being given; to find the radius* CA (r) *of the wheel which ſhall cauſe the weight* π *to aſcend with the velocity* $\frac{d}{r + a} z$.

The exact ſolution of this problem might be deduced from the formula (§ II.) which would render the operation tedious, the equation being of the fourth degree; but it may be rendered far more ſimple by a ſuppoſition which is but little wide of the truth when AB is but ſmall in compariſon of CA, and this is to

conſider

confider all the points of the float-board AB as affected with the fame velocity z.

Let CP fig. 3. $= x$, we fhall have $\frac{nb}{60}(v-z)^2 \int . x\,dx$ for the effect of the portion AP, and $\frac{nb}{60}(v-z)^2 \times \frac{2ar+aa}{2}$ for the effect of the whole float-board AB. This quantity muft be made equal to d π, and then, juft as in the foregoing cafes, fuch a value of z be fought, that the weight π and its velocity may be the greateft poffible; that is, the differential of $z\,(v-z)^2$ muft be made $=0$, which gives $z=\frac{1}{3}v$. Therefore d $\pi = \frac{nabvv}{270}(2r+a)$, and $r=\frac{270\,d\pi - naabvv}{2nabvv}$, and the velocity of the weight π will be $= \frac{2nda bvv}{810 d\pi + 3nbaav} \times v$.

§ VI. We have feen that the calculus was much fimplified by fuppofing one of the velocities conftant for all points of the float-board. For this velocity being c, the effect of the whole float-board will be fimply $\frac{n\beta}{60}(vc)^2\,\frac{aa-ff}{2}$. It will therefore not be unufeful to inquire what this velocity c muft be, that the effect of the float-board may be the fame, as fuppofing, as we have hitherto done, a variable velocity, and proportional to the diftances from the axle, we have only to make $\frac{n\beta}{60}\,\overline{(v-c}^2\left(\frac{aa-ff}{2}\right)$ $=\frac{n\beta}{60}\left(\frac{1}{2}vv\,(aa-ff)-\frac{2}{3}vz\frac{a^3-f^3}{a}+\frac{1}{4}zz\frac{a^4-f^4}{aa}\right)$ (§ IV.) but the equation whence we got the value of z

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§ VI. We have seen that the calculus was much simplified by supposing one of the velocities constant for all points of the float-board. For this velocity being c, the effect of the whole float-board will be simply $\frac{n\beta}{60}(vc)^2 \frac{\alpha\alpha-ff}{2}$. It will therefore not be unuseful to inquire what this velocity c must be, that the effect of the float-board may be the same, as supposing, as we have hitherto done, a variable velocity, and proportional to the distances from the axle, we have only to make $\frac{n\beta}{60}\overline{(v-c}^2\left(\frac{\alpha\alpha-ff}{2}\right)$ $= \frac{n\beta}{60}\left(\frac{1}{2}vv(\alpha\alpha-ff) - \frac{2}{3}vz\frac{\alpha^3-f^3}{\alpha} + \frac{1}{4}zz\frac{\alpha^4-f^4}{\alpha\alpha}\right)$ (§ IV.) but the equation whence we got the value of z

 (§ IV.)

(§ IV.) $\frac{1}{2}vv - \frac{4}{3}\frac{vz}{a}\cdot\frac{a^3-f^3}{a^2-f^2} + \frac{3}{4}zz.\frac{aa+ff}{aa} = 0$, we shall have $c = v - \sqrt{\frac{1}{2}vv - \frac{1}{4}zz.\frac{aa+ff}{aa}}$.

If $f = 0$, we have $zz = 0{,}288vv$ and $c = 0{,}345v$
$f = \frac{1}{2}a$ $zz = 0{,}190vv$ $c = 0{,}336v$
$f = a$ $zz = \frac{1}{9}vv$ $c = \frac{1}{3}v$.

so that whatever be the relation of f to a, the velocity c is ever nearly $= \frac{1}{3}v$, and the more exactly so, as f is greater. Wherefore we may always assume $\frac{1}{270}n\beta vv.(aa-ff) = d\pi$ for the effect of the stream upon a float-board whose plunged part is $a-f$; this effect will be increased in the ratio of 4 to 9, when the wheel has no motion, for making $c = 0$, we find it $= \frac{1}{120}.n\beta vv.(aa-ff)$.

§ VII. Hitherto we have all along supposed, that the float-board did through its whole plunged part receive the perpendicular impulse of the stream; but it is easily understood, that the wheel coming to turn, presents to the stream the plane of the float-board under an angle which is continually varying, which diminishes its effect every instant, as it removes from the vertical: This inconvenience may be remedied by multiplying the number of the float-boards, so that when the first is removed from the vertical as far as a certain point, the next may occupy that advantageous place, to be in its turn replaced some time after by a third, and so on. Now our third inquiry is, to assign the angle contained between two float-boards, or, which comes to the same, the number of float-boards the wheel should consist of, that its effect may be the greatest possible, being of no less importance than the preceding ones. To begin then with the most simple

ſimple caſe; we will ſuppoſe the wheel immoveable, or that $c = 0$, and proceed to inveſtigate, whether, ſuppoſing the number of float-boards to be greater, the ſum of the effects will come out greater or leſs than what reſults from one ſingle float-board placed vertically.

In order to a general ſolution of this queſtion, we will ſuppoſe two float-boards CD and CE fig. 4, making any angles with the vertical, and let us compare the effect of the ſingle float-board GD with the effect reſulting from the float-boards FE and GD taken together, which will be reduced to FE and OD, becauſe the part OG becomes uſeleſs, as the ſtream is intercepted by FE. Let CB = CD = CE = a, CA = f, coſin. BCD = m, coſin. BCE = μ, which gives CG = $\frac{f}{m}$ CF = $\frac{f}{\mu}$ and CO = $\frac{\mu}{m} a$. Then we ſhall find, by § VI, the effect of GD = $\frac{n\beta vv}{120}(mmaa - ff)$ that of OD = $\frac{n\beta vv}{120}(mmaa - \mu\mu aa)$. and that of FE = $\frac{n\beta vv}{120}(\mu\mu aa - ff)$; whence it appears, that the ſum of the two laſt is exactly equal to the firſt, which will ever hold good whatever be the value of f.

Whence ariſes the following theorem:

Whether the wheel be plunged quite up to the axle, or only in part ſo, provided it be immoveable, and that one of its float-boards be placed vertically, its effect will be conſtantly the ſame, whatever be the number of float-boards oppoſed to the ſtream, even though it were infinite.

The

The latter part of this theorem, though flowing from the general demonſtration, may be alſo demonſtrated, immediately, in the following manner; Let BP fig. 5. be $= x$, we have MO $= \frac{\alpha dx}{\alpha - x}$, and CO $= \frac{\alpha\alpha - dx - \alpha x}{\alpha - x}$; $\overline{CO}^2 = \frac{\alpha^4 - 2\alpha^3 x + \alpha^2 x^2 - 2\alpha^3 dx + 2\alpha\alpha x dx}{\alpha\alpha - 2\alpha x + xx}$ (neglecting the dx^2) and $\alpha\alpha - \overline{CO}^2 = \frac{2\alpha\alpha x dx}{\alpha - v}$; Therefore the effect of the ſtream upon OM, which is $= \frac{n\beta vv}{120} \cdot (\alpha\alpha - \overline{CO}^2) \frac{\overline{CP}^2}{\overline{CP}^2}$ will become $= \frac{n\beta vv}{120}$ $(2\alpha x - 2xdx)$ whoſe integral is $= \frac{n\beta vv}{120}(2\alpha x - xx)$ wherein puttting $x = \alpha - f$, we have $\frac{n\beta vv}{120}(\alpha\alpha - ff)$ for the total effect of the ſtream upon the wheel, which is the ſame as that of a ſingle float-board A B in a vertical poſition.

§ VIII. This theorem will alſo hold true for the caſe of § V. wherein we have ſuppoſed the height of the float-boards very ſmall, in compariſon of the radius of the wheel; we have ſeen that the effect of a ſingle float-board placed vertically was $= nabvv$ $(2r + \alpha)$; the demonſtration of the preceding § will be applicable here after the ſame manner, and will ſhew that whatever be the number of float-boards, the effect will be ever the ſame.

It does not however follow that the number of float-boards ſhould be indifferent; for the wheel coming to turn the float-board, its lower part, which received the perpendicular impulſe, will no longer

 receive

receive it otherwise than obliquely, and the effect will diminish till the angle formed by two neighbouring float-boards be bisected exactly by the vertical, which will render the first entirely useless; after which the effect will increase anew, and will become again greatest, when the second float-board is got to the vertical; so that in order to fix upon the most advantageous number of float-boards, regard must be had to the sum of the different effects for all the situations of the float-boards during one whole turn of the wheel.

Whence it follows that in this case, wherein they are supposed very small, the greater their number is, the greater will be the sum total of the effects, since, if that number were infinite, there would be a float-board in a vertical position every instant.

§ IX. This will no longer hold good, if the height of the float-boards be more considerable, and it be found necessary to take the different velocity of their different points into consideration; by comparing (fig. 4.) the pressure on FE with that on the portion GO, they will be found no longer equal, as in the foregoing case; it is true that the same quantity of fluid acts on these two planes, and the disadvantage which FE has by receiving the impulse more obliquely, is exactly compensated, as before, by the length of the lever, but the difference arises from the different velocity of the corresponding points of FE and GO; those velocities are in the ratio of CF to CG, or as cos. ACG to cos. ACF, which shews that the effect of FE is always less than that of GO, and consequently the effect must be diminished, by adding a greater number of float-boards; the

the ſaid effect will be greateſt when there is only one float-board placed vertically, and leaſt when their number is infinite: let us enquire what it will be in this latter caſe. We will ſuppoſe the ſame fig. 5, and the ſame denominations as in § VII. We had $CO = \frac{\alpha\alpha - \alpha x - \alpha dx}{\alpha - x}$, we ſhall have (neglecting dx^2, dx^3, and dx^4) $\alpha\alpha - \overline{CO}^2 = \frac{2\alpha\alpha dx}{\alpha - x}$, $\frac{\alpha^3 - \overline{CO}^3}{\alpha}$, and $\frac{\alpha^4 - \overline{CO}^4}{\alpha\alpha} = \frac{4\alpha\alpha dx}{\alpha - x}$. Now the preſſure on OM is, by § II, $= \frac{n\beta}{120} \frac{\overline{\alpha - x}^2}{\alpha\alpha} \left(vv(\alpha\alpha - \overline{CO}^2) - \frac{4}{3} vz. \frac{\alpha^3 - \overline{CO}^3}{\alpha} + \frac{1}{2} zz. \frac{\alpha^4 - \overline{CO}^4}{\alpha\alpha} \right)$ which (by putting for CO its value) will become $= \frac{n\beta}{120} (2\alpha dx - 2x dx)(v - z^2)$ whoſe integral $\frac{n\beta}{120} (2\alpha x - xx)(v - z^2) = \frac{n\beta\,(v - z^2)}{120} (\alpha\alpha - ff)$ (making $x = \alpha - f$) will expreſs the effect reſulting from an infinite number of float-boards: this leaſt effect will be to the greateſt, that is when there is but one float-board, as $(v - z^2) : vv - \frac{4}{3}\frac{vz}{\alpha} \cdot \frac{{}^3 - f^3}{\alpha\alpha - ff} + \frac{1}{2}\frac{zz}{\alpha\alpha}.(\alpha\alpha + ff)$ or as $(v - x^2) : \frac{1}{2} vv - \frac{1}{4} zz. \left(\frac{\alpha\alpha + ff}{\alpha\alpha}\right)$ §. VI.

This ratio will be that of	1 : 2	if $f = 0$
	1 : 1,485	$f = \frac{1}{2}\alpha$
	1 : 1	$f = \alpha$

§ X. If

§ X. If we take nothing but the moſt advantageous poſition into conſideration, and preſerve the greateſt effect entire, it follows that the angle BCD (fig. 6.) between two float-boards muſt be ſuch that E ſhould enter the water at the inſtant when AB quits the vertical, ſo that the coſine of that angle be $= \frac{f}{\alpha}$; in conſequence of which the following table may be conſtructed, ſhewing what the number of float-boards ſhould be for a given ratio between f and α.

For 4 float-boards, we have $f =$	0,
5	0,3090α
6	0,5000α
7	0,6236α
8	0,7071α
9	0,7660α
10	0,8090α
12	0,8669α
14	0,9009α
16	0,9239α
18	0,9397α
20	0,9510α
&c.	&c.

§ XI. Certain authors treating of hydraulics, have in this part thereof given the ſame table, as containing the true number of float-boards the wheel ſhould conſiſt of: but we have ſeen upon what principle it was formed, and that it was only to preſerve entirely the effect of the vertical float-board; from whence it follows not that the number of float-boards which it aſſigns ſhould be the moſt advantageous.

geous. To which purpose the effect produced from every position of the wheel, and for the different number of the float-boards, should be computed; the number which gives the arithmetical mean between all these effects, the greatest of all, will be that to be chosen, and preferred before what the above table indicates.

It may be sufficiently satisfactory to compute only the effect from 1 to 10 degrees. Thus, for example, for the wheel entirely plunged, we are to find the effect (fig. 7.)

1° on OA, 2° on OI and gb, 3° on OH, and fc, 4° on OG and hd, 5° on OF, and pe, 6° on OE, 7° on OD, 8° on OC, and 9° on OB.

After which the wheel returns into the same position it had at first; and we are to divide the sum of all these effects by 9, to get the arithmetical mean.

We will next suppose the number of six float-boards for the same case of $f = 0$, and compute the following effects.

1° on OG + aA, 3° on OE + nc, 5° on OI + γC
2° OF + mb, 4° OD 6° OH + βB.

The sum of all these effects divided by 6 will give the effect of the wheel of 6 float-boards.

The same thing, supposing the angle 40 degrees, or 9 float-boards, and as after a revolution of these 40 degrees, the wheel returns into a similar position, the same must be divided by 4.

Then for an angle of 30 degrees we are to divide by 3, and so on.

for

I have made this computation to great exactneſs, for the caſe of $f = 0$, $f = \frac{1}{2}\alpha$, and $f = 0,866\,\alpha = \alpha$ coſ. 30°; the reſult,

1° If $f = 0$.

for 4 float-boards, the arith. mean $= 0{,}335$ ($\frac{1}{16}$ $n\beta\alpha\alpha vv$).

It may be obſerved in this firſt caſe, that there is ſome advantage in taking 6 float-boards inſtead of 4 ſhewn by the table; the effect will be increaſed in the ratio of 100 to 118, and yet will be more than about $\frac{92}{100}$ of the greateſt effect above calculated for a ſingle vertical float-board; ſo that the found dimenſions muſt be a ſmall matter altered, and the quantity $\beta\alpha\alpha$ increaſed by $\frac{1}{10}$.

for 6	$= 0{,}396$ (&c.
9	$= 0{,}336$ (&c.
12	$= 0{,}323$ (&c.
18	$= 0{,}295$ (&c.
an infinite number	$= 0{,}214$ (&c.

2° If $f = \frac{1}{2}\alpha$.

for 6 flo.	0,277 (&c.
7	0,281 (&c.
9	0,285 (&c.
12	0,284 (&c.
18	0,276 (&c.
an infinite number	0,238 (&c.

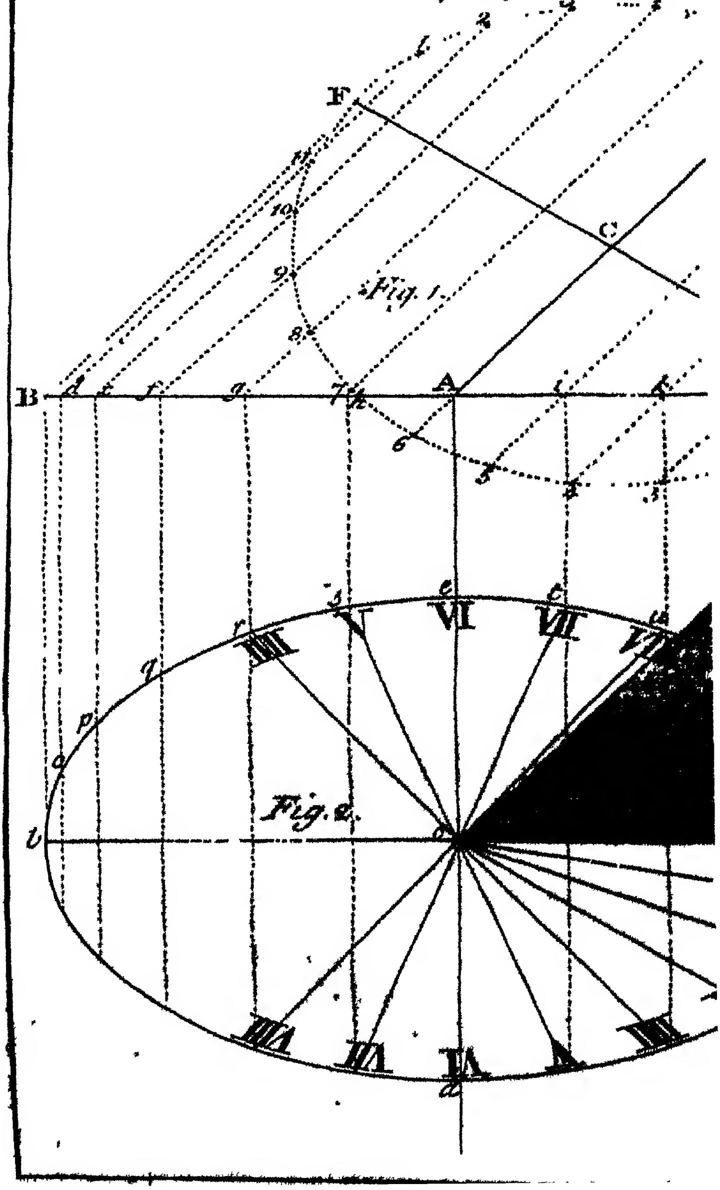
In this ſecond caſe 9 float-boards are to be taken inſtead of 6 ſhewn by the table, though the difference will be but very ſmall; and we ſhall have an effect which will be $\frac{86}{100}$ of that of a vertical float-board, and in that ratio that the quantity $\beta\alpha\alpha$ found by the above formulæ, muſt be increaſed.

3° If $f = 0,866\,\alpha$

for 12 fl.	= 0,099 (&c.
18	= 0,099 (&c.
36	= 0,104 (&c.
an infinite number	= 0,103 (&c.

In this third caſe the difference is ſtill very ſmall, and the effect reſulting from 36 float-boards will be $\frac{93}{102}$ of the effect of a ſingle vertical float-board.

Philos. Trans. Vol. LXIII. TAB. XVI. p. 380.



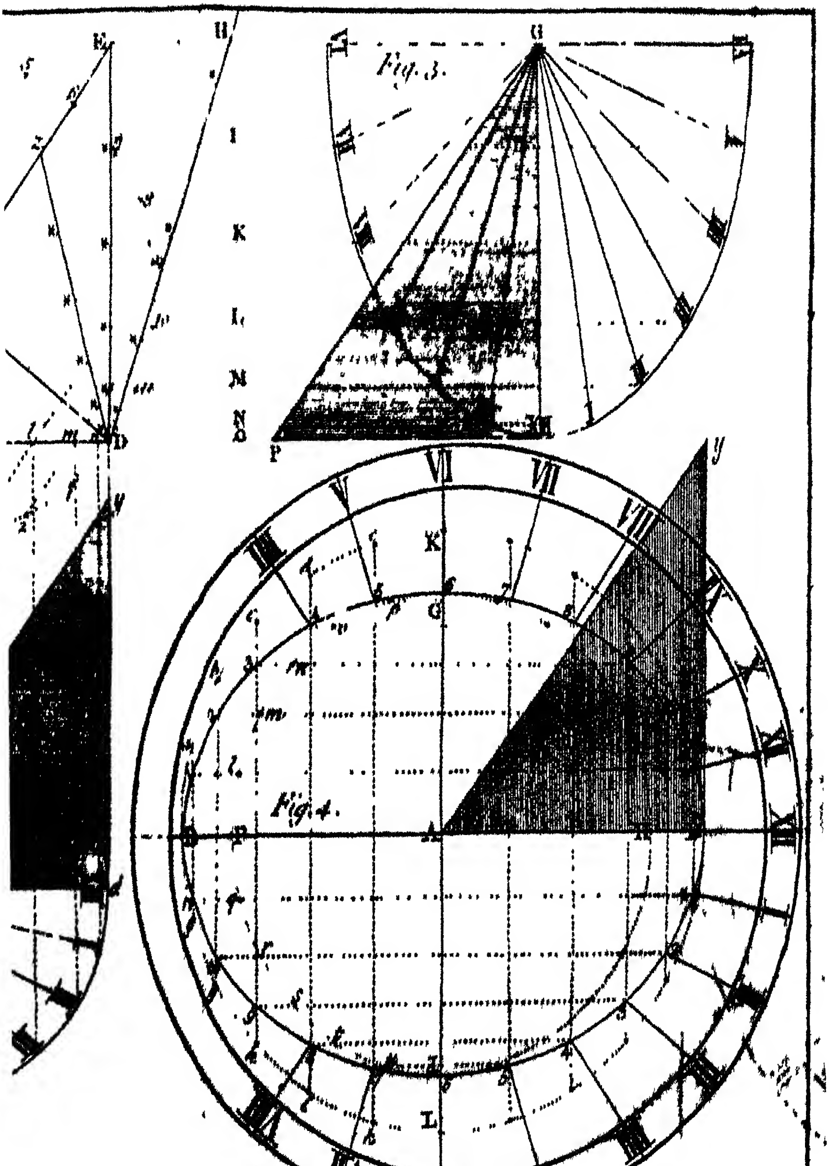


Fig. 3.
Fig. 4.

XXXVI. *A new Method of constructing Sun-Dials, for any given Latitude, without the Assistance of Dialing Scales or Logarithmic Calculations.* *By* James Ferguson, *F. R. S.*

Read July 2, 1767.

DRAW the straight horizontal line BAD TAB. XVI. (fig. 1.) of any convenient length, and on the end D thereof raise the perpendicular DE.

Bisect BAD at A, and draw the right line ACE, making the angle BAD equal to the latitude of the place for which the dial is to serve, as suppose 51° ½ for the latitude of London. Draw also the right line FCD, making an angle at D with the horizontal line BAD equal to the [illegible] itude of the place, [illegible] height of the [illegible] FCD will be perpendicular to [illegible] ACE, BAD will be a horizontal plane seen edgewise, DE a vertical plane, FCD the plane of the equinoctial, and ACE the axis or stile of the dial; the whole triangle ADE representing the whole [illegible] plate or stile.

Around this intersection C, as a center, with the radius CD, describe the circle F 6 D 6 F, and divide its circumference into 24 equal parts, beginning at D or at F. Then connect all the points of division, which [illegible] from F, by the straight lines [illegible] 11, 2 10, 3 [illegible] continuing as many [illegible]

[illegible] 4 [illegible]

theſe lines as are needful to the horizontal line BAD, and to the vertical line DE.

Continue ED down to *d* (fig. 2.) and draw *bd* parallel and equal to BD. Draw alſo the right line A*eca*, from fig. 1. to fig. 2, and that the line will be perpendicular to the line *bd* in fig. 2. cutting it in the middle point *c*.

From fig. 1. take CF or CD in your compaſſes; and in fig. 2. ſet that diſtance from *c* to *e* upwards, and from *c* to *a* downwards, on the right line *Aeca*. So *eca* in fig. 2. ſhall be equal to FCD in fig. 1. and *bcd* in fig. 2. ſhall be equal to BAD in fig. 1.

On theſe two lines *bcd* and *eca* make the ellipſis *bopqr*, &c. according to the common rule for deſcribing an ellipſis upon the tranſverſe and conjugate diameters *bcd* and *eca*. Then, from thoſe points in the horizontal line BAD (fig. 1.) where the right lines, 1 11, 2 10, 3 9, &c. meet it, as at *d*, *e*, *f*, *g*, *h*, *A*, *i*, *k*, *l*, *m*, *n*, draw the right lines *do*, *ep*, *fq*, *gr*, &c. quite through the ellipſis, and all parallel to the right line *Aeca*. Then, from the middle point *c* of the ellipſis, draw right lines to thoſe points of its circumference where the foreſaid parallel lines cut it; and they ſhall be the true hour-lines for a horizontal dial; to which ſet the hours, as in fig. 2. Laſtly, in fig. 2. draw *cy* parallel to ACE in fig. 1. and *cy* ſhall be the axis or edge of the ſtile *cdy* that caſts the ſhadow on the time of the day.

The horary ſpaces, or angular diſtances of the hours on the dial, being thus found, there is no occaſion for confining the hours thereon, or the lengths of the hour-lines, within the ellipſis; for they may

be

produced beyond it to any diſtance, and the hour-letters placed in a circle, as in fig. 4.

A geometrical method for deſcribing the whole or half an ellipſis will be ſhewn further on.

From fig. 1. contiue out the horizontal line BAD to any length, as to XII in fig. 3. Then, from the points *** in the perpendicular DE (fig. 1.) where the parallel lines 5 7, 4 8, 3 9, 2 10, and 1 11 meet it, draw the right lines H, I, K, L, M, N, all parallel to the horizontal line BA DP XII. producing them at pleaſure: and, in fig. 3. draw G XII parallel to DE in fig. 1. This done, take CF or CD (fig. 1.) in your compaſſes, and ſet off its length both ways from G (fig. 3.) to VI and VI, on the right line *EH* VI *G* VI. So VI *G* VI in fig. 3. ſhall be equal to FCD in fig. 1. and XII *G* in fig. 3. ſhall be equal to DE in fig. 1.

On VI *G* VI as a conjugate diameter, and *G* XII as a ſemi-tranſverſe diameter, deſcribe the ſemi-ellipſe VI, VII, VIII, [illegible] it, draw the right lines [illegible] *G* VII, *G* VIII, *G* IX, &c. as in the figure; and they will be the true hour-lines for an erect direct ſouth dial: and they may be produced beyond the ellipſis, and limited [illegible] or ſquare lines, between which the [illegible] may be placed.

Laſtly, draw PG in fig. 3. parallel to [illegible] 1. and PG will be the axis or edge of the [illegible] XII *G* [illegible] ſhadow [illegible]

And [illegible] ructed for any given [illegible] or vertic[illegible] may be made for [illegible]

If

If you want a ſouth dial to incline by any number of degrees, as ſuppoſe 16, draw the line D*z*, making an angle of 16 degrees with the perpendicular DE, in fig. 1. Then D*z* ſhall be the ſemi-tranſverſe axis of the ellipſe, and C6 the ſemi-conjugate: and right lines drawn parallel to DP XII quite through the ſemi-ellipſis, from the points *** in D*z*, where it is cut by the parallel lines 5 7, 4 8, 3 9, *&c.* ſhall cut the ſemi-ellipſis in thoſe points through which the hour-lines muſt be drawn, as from G in the upright ſouth dial, fig. 3.

If you want to make a reclining ſouth dial, draw the line DH (fig. 1.) making an angle with the perpendicular DE equal to the intended angle of reclination, and produce DH and CE till they meet. From D to that meeting, will be the length of the ſemi-tranſverſe axis of the ellipſe, and from C to 6 the length of the ſemi-conjugate: which being found, proceed in all reſpects as above for the ſouth upright dial.

To draw the ellipſis, and find the hour-points in it, obſerve the following method.

For a horizontal dial, as fig. 4. Make the radius AK of the circle BKDL equal to AD in fig. 1. and croſs the circle at right angles, by the two diameters BAD and KAL, and divide the circle into 24 equal parts, beginning at B. Connect theſe points of diviſion, which are equidiſtant from B, by the right lines *af*, *bg*, *ch*, &c. all parallel to KAL, as in the figure.

Make the radius AG of the circle FGHI in fig. 4. equal to CF in fig. 1. and divide FGHI into 24 equal parts, beginning at I. Then through theſe

points

points of diviſion, which are equidiſtant from I, draw the right lines 7 5, 8 4, 9 3, 10 2, *&c.* till they meet the former right lines, *ek*, *di*, *cb*, *&c.* in the points 7 5, 8 4, 9 3, 10 2, and 11 1, on both ſides of the diameter BAD; all which points are in the elliptical curve, and it is to be drawn through them, by hand, as in the figure.

And right lines drawn from the center A through theſe points in the ellipſis, will be the true hour lines for a horizontal dial.

To draw the ellipſis for a vertical ſouth dial, make DE (in fig. 1.) the radius of the largeſt circle, and CF the radius of the ſmalleſt: the diameter of the former gives the tranſverſe diameter of the ellipſis, and that of the latter gives the conjugate: which being found, conſtruct the ellipſis the ſame way for the vertical dial as above ſhewn for the horizontal; then draw the hour-lines in the ſame manner, from the center of the dial, through thoſe points of the ellipſis where the interſections of the croſs-lines meet it, as in the horizontal; and the thing will be done.

XXXVII. *On the Formation of Iſlands.* *By* Alexander Dalrymple, *Eſquire.* *Communicated by* C. Morton, M. D. *S. R. S.*

Received May 4, 1767.

Read July 2, 1767.

THERE is not a part of natural hiſtory more curious, or perhaps to a navigator more uſeful, than an enquiry into the formation of iſlands. The origin of iſlands, in general, is not the point to be diſcuſſed; but of low, flat, iſlands in the wide ocean; ſuch as are moſt of thoſe hitherto diſcovered in the vaſt South-ſea.

Theſe iſlands are generally long, and narrow; they are formed by a narrow bar of land, incloſing the ſea within it; generally, perhaps always, with ſome channel of ingreſs at leaſt to the tide; commonly, with an opening capable of receiving a canoe; and frequently ſufficient to admit even larger veſſels.

The origin of theſe iſlands will explain their nature. What led me firſt to this deduction was an obſervation of Abdul Roobin, a Sooloo pilot; that all the iſlands, lying off the N.E. coaſt of Borneo, had ſhoals to the eaſtward of them.

Theſe iſlands being covered to the weſtward by Borneo; the winds from that quarter do not attack them

them with violence. But the N.E. winds, tumbling in the billows from a wide ocean, heap up the coral with which thofe feas are filled. This, obvious after ftorms, is perhaps, at all other times, imperceptibly effected.

The coral banks, raifed in the fame manner, become dry. Thefe banks are found of all depths, at all diftances from fhore, entirely unconnected with the land, and detached from each other: although it often happens they are divided by a narrow gut, without bottom.

Coral banks alfo grow, by a quick progreffion, towards the furface; but the winds, heaping up the coral from deeper water, chiefly accelerate the formation of thefe into fhoals and iflands. They become gradually fhallower; and, when once the fea meets with refiftance, the coral is quickly thrown up by the force of the waves breaking againft the bank; and hence it is that, in the open fea, there is fcarce an inftance of a coral bank having fo little water, that a large fhip cannot pafs over, but it is alfo fo fhallow that a boat would ground on it.

I have feen thefe coral banks in all the ftages; fome in deep water, others with few rocks appearing above the furface, fome juft formed into iflands, without the leaft appearance of vegetation, and others, from fuch as have a few weeds on the higheft part, to thofe which are covered with large timber, with a bottomlefs fea, at a piftol fhot diftance.

The loofe coral, rolled inward by the billows in lage pieces, will ground, and the reflux being unable to carry them away, they become a bar to coagulate the fand, always found intermixed with

coral; which ſand, being eaſieſt raiſed, will be lodged at top. When the ſand bank is raiſed by violent ſtorms, beyond the reach of common waves, it becomes a reſting place to vagrant birds, whom the ſearch of prey draws thither. The dung, feathers, &c. increaſe the ſoil, and prepare it for the reception of accidental roots, branches, and ſeed, caſt up by the waves, or brought thither by birds. Thus iſlands are formed: the leaves and rotten branches, intermixing with the ſand, form in time a light black mould, of which in general theſe iſlands conſiſt, more ſandy, as leſs woody; and when full of large trees, with a greater proportion of mould.

Cocoa nuts, continuing long in the ſea without loſing their vegetative powers, are commonly to be found in ſuch iſlands; particularly as they are adapted to all ſoils, whether ſandy, rich, or rocky.

The violence of the waves, within the Tropicks, muſt generally be directed to two points, according to the monſoons.

Hence the iſlands formed from coral banks muſt be long and narrow, and lie nearly in a meridional direction. For even ſuppoſing the banks to be round, as they ſeldom are when large, the ſea, meeting moſt reſiſtance in the middle, muſt heave up the matter in greater quantites there than towards the extremities: and, by the ſame rule, the ends will generally be open, or at leaſt loweſt. They will alſo, commonly, have ſoundings there, as the remains of the bank, not accumulated, will be under water.

Where the coral banks are not expoſed to the common monſoon, they will alter their direction; and

and be either round, extend in the parallel, or be of irregular forms, according to accidental circumſtances.

The interior parts of theſe iſlands, being ſea, ſometimes form harbours capable of receiving veſſels of ſome burthen, and, I believe always, abound greatly with fiſh; and ſuch as I have ſeen, with turtle-graſs and other ſea-plants, particularly one ſpecies, called by the Sooloos Gammye, which grows in little globules, and is ſomewhat pungent, as well as acid, to the taſte.

It need not be repeated, that the ends of thoſe iſlands, only, are the places to expect ſoundings: and they commonly have a ſhallow ſpit running out from each point.

Abdul Roobin's obſervation points out another circumſtance, which may be uſeful to navigators; by conſideration of the winds to which any iſlands are moſt expoſed, to form a probable conjecture which ſide has deepeſt water; and from a view which ſide has the ſhoals, an idea may be formed which winds rage with moſt violence.

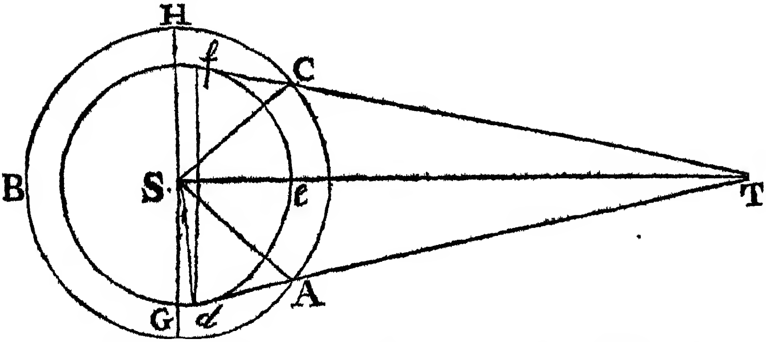
XXXVIII. *An Attempt to determine the Height of the Sun's Atmoſphere from the Height of the Solar Spots above the Sun's Surface: In a Letter to Mr.* J. Ellicot, *F. R. S. from the Rev. Mr.* Horſeley, *F. R. S.*

Read July 9, 1767.

I Return you many thanks for your obliging communication of the obſervations of the late tranſit of Venus by Mayer and Rohlius. The phænomena which they relate of the atmoſphere of that planet are highly curious. They were perhaps the more intereſting to me, as they confirmed ſome conjectures of my own, concerning the great height of the atmoſphere of the ſun, and of thoſe of the two nearer planets. I once attempted to make a rough compariſon between the height of the ſun's atmoſphere and that of our own, by comparing the height of the ſolar ſpots above the ſurface of the ſun with that of our clouds above the ſurface of the earth, which I did in the following manner.

The inclination of the Sun's equator to the place of the earth's orbit is ſo ſmall, that in this enquiry I think it may ſafely be neglected; and I conſider the two planes as one. Let T be the center of the earth, S that of the Sun. Join T S, and let *def* be a great circle of the ſun's ſphere, formed by the interſector of the plane of the earth's orbit with the ſun's ſurface.

face. Let ABC be the circle deſcribed by the revolution of a ſpot. From T draw T*f* and T*d* touch-



ing the circle *def* in *f* and *d*, and cutting ABC, in C and A. Join *df*; through S draw HSG, parallel to *df*. Join SC, SA, S*d*. The ſpots are hid behind the ſun three days longer than they are viſible. That is, they are hid 15 days, and are ſeen only 12. The earth's motion in 15 day is 14° 17′. The ſpots traverſe the like area in 1 d. 0 h. 50′ nearly. Hence, if the earth ſtood ſtill, the ſpots would be hid only 13 d. 23 h. 10′, and their whole ſidereal period being 25 d. 5 h. they would be viſible 11 d. 5 h. 50′, and the time of their occultation would exceed the time of their appearance by 2 d. 17 h. 20′. Hence the arc AC is leſs than the arc ABC, by the motion of 2 d. 17 h. 20′, that is, by 38° 52′ 56″. And the ſemi-circle being a mean arithmetic between AC and ABC, AC will be leſs than the ſemi-circle by half as much; that is, by 19° 26′ 28″. Hence each of the angles GSA, HSC is 9° 43′ 14″. The angle *bsd* = *d*TS = 16′ 1″ 27‴. Therefore *d*SA is 9° 27′ 12″. Hence SA = 1,013767 ſuch parts as S*d* is 1.

The

The distance therefore of these spots from the center of the sun is 1,013767 semi-diameters of the sun, and their distance from his surface is in in decimal parts of his semi-diameter ,013767. Hence it is evident that the height of the solar spots above the surface of the sun, is above 54 such parts, as bear each to the sun's semi-diameter, the proportion of one Paris mile to the semi-diameter of the earth, which is that of 1 to 3923 nearly. The height of our atmosphere is generally reckoned about 50 miles. That of the lightest clouds fall short of one mile. The whole height of our atmosphere therefore is, at least, 50 times that of our highest clouds. If the whole height of the sun's atmosphere bear as large a proportion to the height of these solar spots or clouds (and I think the proportion is likely to be much larger), the height of the sun's atmosphere is not less in proportion to his semi-diameter, than 54 times that of the earth's, and exceeds two thirds of his semi-diameter, being in decimal parts thereof ,68835.

The probability seems to be that the height of the sun's atmosphere is almost double of this: for I question whether the mean height of our clouds exceeds ½ a Paris mile. The solar spots, therefore, are 108 times as high in proportion; and then, supposing as before, that the whole height of the sun's atmosphere bears the same proportion to the height of his spots, as the whole height of our atmosphere to the mean height of our clouds, the sun's atmosphere will be 108 times as high in proportion to his semi-diameter as ours is, and will rise

to the diſtance of more than $\frac{4}{5}$ of his ſemi-diameter from his ſurface.

Let philoſophers conſider, whether theſe indications of the vaſt height of the ſun's atmoſphere give any degree of probability to a conjecture of Sir Iſaac Newton's, that the diſſipation of the ſun's ſubſtance, which might be expected to enſue from his intenſe heat, may in great part be prevented by the prodigious preſſure of the incumbent atmoſphere.

The height of the atmoſphere of Venus is conſiderably greater according to the obſervations of Mayer and Rohlius than they imagined. Rohlius follows Caſſini in the eſtimation of the ſun's apogee ſemi-diameter, which Caſſini over rated by $3'' 45'''$. This quantity, therefore, is to be added to the height of Venus's atmoſphere (15,5) as ſtated by Rohlius; which makes the true height $19''.25$, that is above $\frac{1}{3}$ of the diameter of the planet. I cannot but reflect with ſome degree of national triumph on the great part that our own country may juſtly claim in many of the moſt curious diſcoveries in all parts of the world. Mr. Meyer generouſly confeſſes how much he ſtood indebted to Engliſh artiſts. You told me that it is your intention to preſent that curious tract to the Royal Society. You may likewiſe communicate this if you think it contains any thing worthy of their notice.

I am, Sir, with great eſteem,

Your moſt obedient

and moſt humble ſervant,

Broad-Street,
June 11, 1767.

Sam. Horſley.

XXXIX. *Observations of the Sun's Eclipse, 16th of* August, 1765, *taken at* Caën *in* Normandy. *By* Nathanael Pigott, *Esquire, of* Whitton, *in* Middlesex. *Communicated by* J. Bevis, *M. D. F. R. S.*

Read July 9, 1767.

Tr. Time.

	h	′	″			′	″	‴
At	3	57	28	the seg. of the sun's illum. diam.	meas.	29	38	14
	4	8	52	the distance of the horns	ditto	14	47	37
	4	18	39	the seg. of the illuminated diam.	ditto	27	4	35
	4	24	28	the distance of the horns	ditto	16	20	24
	4	35	47	the seg. of the illuminated diam.	ditto	27	14	14
	4	43	4	the distance of the horns	ditto	14	26	6
	4	52	38	the seg. of the illuminated diam.	ditto	29	52	1
	4	56	54	the distance of the horns	ditto	7	46	4

		h	′	″	′	″	‴	
Sun's incl. diam. meas.	at	3	19	38	31	45	11	the mean ′ ″ ‴ 31 43 20 of the Sun's diam. meas.
—— Ditto ——	at	3	22	10	31	42	58	
Sun's horiz. diam. aft. the Eclipse	at	5	31	33	31	41	29	
Sun's inclined diam. meas. August 15th	at	3	53	37	31	43	42	

Eclipse

	h ′ ″		h ′ ″
Eclipse beg. tr. time at	3 48 16	hence the mid. was at	4 24 36
——— end. at	5 0 56½	and greatest phase obs. at	4 18 39
——— mid. at	4 24 36	whence the ecl. incr. for	5 57 of
——— dur. at	1 12 40½		

time, in which the Sun's diam. illum. decreased 36″ 14‴; therefore from the Sun's diam. illuminated at

	h ′ ″	′ ″ ‴
fore from the Sun's diam. illuminated at	4 18 39	=27 4 35
take the decrease in	+ 5 57	= 0 36 14
the diam. of the Sun at the middle	4 24 36	=26 28 21

which taken from the mean diam. meas. 31′ 43″ 20‴ gives 5 14 59

the quantity of the eclipse, or segment of the diameter eclipsed, which is 1 digit and 59′,15 of a digit, or 1 $\frac{59}{60}$ = 2 digits nearly. This eclipse was observed with an achromatic refractor of 6 feet, and a micrometer made by Dollond. The weather very fine.

The times, as computed from the Tables at the end of M. De la Lande's Astronomy.

	h ′ ″		h ′ ″	difference. ′ ″
Beginning at	3 48 24,6	observ. at	3 48 16	0 8,6
Middle	4 25 11,0	———	4 24 36	0 35,0
End	5 1 57,2	———	5 0 56,5	1 0,7
Duration	1 13 32,6	———	1 12 40,5	0 52,1

Also the latitude of the Moon was, by observation, 16″ greater than the tables gave it.

XL. *Extract of a Letter from* John Ellis, *Esquire, F. R. S. to Dr.* Linnæus, *of* Upsal, *F. R. S. on the Animal Nature of the Genus of Zoophytes, called Corallina.*

Read July 9, 1767.

I HAVE now finished a collection of that genus of Zoophytes, which you call Corallina; and, with the assistance of our learned friend Dr. Solander, have made a description of each species: to do this with more exactness, I have taken care to dissect them minutely, and to pass them in review under his eye in the microscope, in order to establish a true general character of this genus.

I have attended more particularly to examine the nature of these bodies, in order to confute the opinions of some late writers on Zoophytes, who, for want of good microscopes, and a proper care in chemically analysing them, have asserted that they were mere vegetables.

The first of these is Dr. Job Baster, of Zeeland, who, in the Philosophical Transactions, Vol. LII. p. 111, asserts that the Corallines of Linnæus, which he says he has accurately examined, are most evidently true plants of the genus of Conferva; because there are no polypes coming out of their tops, and that they have seed inclosed in their cells like other ma-

rine

rine plants *. But, as another part of this letter is intended for an inquiry into this new discovery of Dr. Baster's, that Corallines are Confervas; a thing never known even to the great Mr. Ray, Dr. Dillenius, or any other botanist, I shall now proceed to his ingenious friend Dr. Pallas of Berlin, who has lately resided in Holland, and who has taken great pains in collecting every thing that has been wrote on the subject of Zoophytes, from whence he has compiled a book called Elenchus Zoophytorum, where he has ranged the several genera and species of this class of beings in a systematical order.

When he comes to the genus of Corallina, he says (vide Pallas Elenchus, p. 418.) †, "They are to "be left to the botanists, as they belong to the vegeta-"ble kingdom; but makes this apology for inserting "them, least his book should be thought imperfect, "as Linnæus and Ellis have ranked them as "Zoophytes in their works."

* Corallinas, non Zoophyta, quamvis Linnæus iisdem adnumeret, sed veras e confervarum genere plantas esse, luculentissime perspexi. Nunquam in earum apicibus polypi inveniuntur: semen contra cellulis inclusum eodem quo aliæ plantæ marinæ modo produnt. Phil. Transf. Vol. LII. p. 3.

† Corallinas ad vegetabilia referendas esse. Mihi vero totum hocce genus botanicis relinquendum videtur. Nec enim structurâ, nec chymicis principiis ad Zoophytorum ullum genus accedunt, et pleræque species etiam habitum prorsus peculiarem habent, aliquæ ad fucos potius accedentes, plurimæ confervis comparabiles, quamvis lapidescenti substantia ab iisdem et omnibus vegetabilibus distinctissimæ. Pallas Elenchus. Zoophyt. p. 418.

He

He begins with obſerving, that they don't come near to any one genus of Zoophytes, neither in their ſtructure nor chemical principles; that ſome ſpecies have a peculiar appearance, ſome approach to Fucus's, many are like Confervas; but that all of them are very diſtinct from them, and from all vegetables, on account of their lapideſcent ſubſtance.

That they differ in their chemical principles from Zoophytes; for when they are burnt, they ſmell like vegetables: and that, according to Count Marſigli's Experiments (Hiſt. Mar. p. 73.) they neither contain a volatile ſalt, or animal oil.

That the pores, in their calcareous ſubſtance, are too ſmall for polypes to inhabit them; and that the pores of Fucus's prove them to be as much animals as the Corallines, even when their pores are rendered more viſible, by having the calcareous ſubſtance, that ſurrounds them, diſſolved by an acid.

That the great Juſſieu, in his diligent reſearches after marine productions could ſee no viſible token of life in them.

That Mr. Meeſe, who has lately wrote a Flora Friſica, has found a Coralline growing upon a heath in Frieſland; which, Dr. Pallas ſays, is a ſtrong proof of their vegetable origin.

Laſtly, that their fructification is ſo nearly analagous to thoſe of Fucuſes and Confervas, that he likewiſe takes that to be a proof of their belonging to the vegetable kingdom.

To proceed then.—Dr. Pallas, after telling us that Corallines are vegetables, ſays, that ſome of them are like Fucuſes.

In

In this I muſt agree with him; becauſe his firſt Coralline, which he calls Corallina Pavonia, is truly of that genus of plants: this moſt elegant Fucus I have particularly deſcribed and figured (Eſſay on Corall. p. 88. T. 33. fig. *c*, *d*, *e*,); it is well known by the name of Turky-feather Fucus, and is called, in the Species Plant. p. 1630, Fucus Pavonius. What could have led Dr. Pallas into this miſtake? moſt probably thoſe beautiful farinaceous ſemi-circular ſtripes on it, which he muſt have taken for a lapideſcent or calcareous ſubſtance *, one of the moſt diſtinguiſhing characters of a Coralline, even according to his own deſcription of this genus. If he had tried this farinaceous ſubſtance with an acid, he might obſerve, that it would not ferment; it is of the ſame nature with the farina that covers many plants, for inſtance the Primula Auricula, and almoſt all the Lichenes foliacei and fruticuloſi, or Liverworts. As to their ſimilitude to the Conferva, the contrary will appear, as ſoon as I come to give the proper definitions to both theſe, and the Corallines. In the ſame paragraph he ſays, that the Corallines do not come near to any genus of Zoophytes.

How far he is miſtaken in this aſſertion, I will endeavour to prove from the following experiments.

Break a thin piece from the Corallium Anglicum, Eſſay on Corall. T. 27. N. 1. *c*. (Millepora Calcarea, Pallas Elench. p. 265.) or of the Corallium Lichenoides, Eſſay on Corall. T. 27. N. 2. *d*.; both

* Quamvis lapidiſcenti ſubſtantia ab omnibus vegetabilibus diſtinctiſſimæ. Pallas, Elench. 418.

which, Dr. Pallas, in his Elench. p. 265. has confounded together under the name of Millepora Calcarea (but which he confeffes to be animal); and when you examine them in the microfcope, you will find in them both regular feries of cells, as figured in Effay on Cor. Tab. 27. Fig. D. Split at the fame time one of the joints of the Corallina Officinalis of Linnæus lengthways, and you will find the feries of cells * correfpond in fhape exactly with both the former; which I think proves the organization of thefe bodies to be the fame, and confequently animal.

Befides thefe, compare the ftructure of the Miriozoon of Donati, Phil. Tranf. Vol. XLVII. p. 107. Tab. 5. (Millepora truncata, Pallas Elench. p. 249.) with thofe of the Corallina Rofarium, and Corallina incraffata, both which I have carefully diffected and figured in Tab. XVII. Fig. 15, 20, &c. and there appears fo great an affinity between their cells (and even in the opercula of the Corallina incraffata), that it affords us reafon to conclude with great probability, that their mouths, or fuckers, are the fame. It cannot be amifs to mention the fimilitude there is between the ftony-jointed Corallines, and the Ifis Hippuris, or jointed black and white Eaft Indian Coral, and the Cellularia Salicornia, Pallas Zooph. p. 61. or Bugle Coralline, Effay on Coral. T. 23. which two laft are univerfally allowed to be animals: in all thefe are found the fame kind of fibres that connect their joints, and exactly in the fame manner.

In order to prove that thefe Corallines have a fmell very different from vegetables, I muft appeal to

* See Tab. XVII. fig. 14 and 13.

an experiment made publickly before the Society of Arts, Commerce, &c. and which gave them a satisfactory demonstration of the great difference in nature between Corallines and vegetable substances. It happened upon the following occasion. A gentleman of Wales had sent the society a parcel of Lichen tartareus, of Linn. Ed. 2. Sp. Pl. 1608. as a proper material for dying a red colour, to answer the same purpose of that expensive article among the dyers, called Orchell, or Canary weed, which is the Lichen Roccella of Linn. Sp. Pl. 1622.

As the object was of consequence, the society was very desirous of being fully informed of the nature and appearance of this useful dye; and therefore, several curious gentlemen of the society were desired, against the next meeting, to bring some specimens of true Orchell. Accordingly some specimens were obtained from the Orchell dyers in Southwark, and laid before the society.

At the same time Dr. Maningham, a member of that society, produced before the society a specimen, in a paper with Orchell wrote upon it, from Mr. Miller of Chelsea, likewise as the true Orchell: but, upon examining it, it proved to be the Corallina nervo tenuori fragiliorique internodia nectente of Sir Hans Sloane's History of Jamaica, Vol. I. Tab. 20. Fig. 4. Some disputes arising on the different appearance of the specimens, I took the liberty to inform the gentlemen present, that, having lately made some experiments on Corallines, I believed that Mr. Miller's specimen was a Coralline, or animal substance, and the Lichen Roccella, or Dyers Orchell, was a vegetable; and in order to convince the society of the

 difference,

difference, I called for a lighted candle, and having first set fire to the Lichen Roccella, it yielded the same smell that burnt vegetables usually do; but when the Coralline (which was Mr. Miller's specimen) was burnt, it filled the room with such an offensive smell like that of burnt bones, or hair, that the door was obliged to be opened, to dissipate the disagreeable scent, and let in fresh air.

Another argument that Dr. Pallas offers the world of the vegetable nature of Corallines, or rather a proof of their not being of an animal nature, are Count Marsigli's Chemical experiments on the Corallina Officinalis (Hist. Mar. p. 73.) where he says it neither contains animal oil nor volatile salts.

But, to prevent such plausible arguments from misleading mankind, I determined to have fair and accurate experiments made on this substance. Accordingly I applied to Mr. Peter Woulfe, F. R. S. a gentleman distinguished for his great knowledge in chemistry; and in order to have the specimens fresh from the sea, I applied to a worthy member of this Society, the Right Honourable the Earl of Hillsborough, for Mr. Potts, the Secretary to the Post-Office, to procure me a sufficient quantity of the Corallina Officinalis from the sea-coast near Harwich: this parcel, about two months ago, I sent to Mr. Woulfe; and in answer have received the following letter, with an account of his experiments made on it.

Clerkenwell.

Clerkenwell, May 5, 1767.

SIR,

I TOOK twelve ounces troy of the Corallina Officinalis (which you ſent me) picked clean from every extraneous ſubſtance, and put it into a clean ſtone-coated retort; the retort was ſet in a reverberatory furnace, and an adopter and quilled receiver luted to it: the fire was very gentle for the firſt eight hours; in which time, half an ounce and eighteen grains of a tranſparent and almoſt colourleſs liquor came over, which was ſet aſide for examination. The fire was then increaſed, and in ſix hours time there were diſtilled two drams and three grains of a turbid liquor, which had ſome appearance of oilineſs on its ſurface; this was likewiſe ſet a-part to be examined. The fire was then increaſed for ſix hours longer, and during the laſt two hours the retort was quite red hot all over, which ended the diſtillation. In this third and laſt proceſs the portion of liquor that came over was more turbid than the ſecond, and ſome of it from the redundancy of its volatile alkaly was cryſtallized; it alſo contained rather more than a dram of light empyreumatic oil, very much reſembling the ſmell of hartſhorn; in the recipient there was alſo ſome cryſtals of a volatile alkali. The whole of this laſt product weighed three drams and an half. The caput mortuum was quite black, and weighed ten ounces, one dram, and one ſcruple; ſo that there was a loſs of four drams and forty-nine grains out of the twelve ounces of Coralline.

The firſt liquor that diſtilled ſlightly effervefced with ſpirit of ſalt, and changed ſyrup of violets green, certain proofs of a volatile alkali.

The ſecond and third portion effervefced ſtrongly with ſpirit of ſalt, as did alſo the volatile ſalt that came over into the receiver, evident marks of its being a concentrated alkali.

Here I muſt obſerve, that had this diſtillation been conducted in a hurry, there would have been no concrete volatile alkali; for then this would have been confounded and diſſolved in the firſt liquor that came over.

Had there been a ſufficient quantity of this Coralline, I ſhould firſt have propoſed to have taken off the calcareous ſubſtance, by an acid menſtruum, and afterwards waſhed the membranaceous part ſo clean from the acid, as not to change the ſyrup of violets red.

Then the diſtillation of this part alone would have afforded a much larger proportion of empyreumatic oil, and volatile alkali, and but a very ſmall quantity of caput mortuum.

If you think theſe experiments of any uſe, you have my free leave to lay them before the Royal Society.

I am, Sir, yours, &c.

To John Ellis, Eſq;
in Grays Inn.

Peter Woulfe.

Doctor Pallas proceeds to prove that Corallines cannot be animals, as the * pores of their calcareous ſubſtances

* Pori autem calcareæ ſubſtantiæ ita ſunt minuti, ut polypi in iis hoſpitari nequeant. Pall. Elench. p. 419.

fubftances are too minute for any polypes to harbour in. Thefe words of the Doctor's feem to imply, as if the Coralline fubftances were only habitations for detached polypes, and not part of the animals themfelves. How this affair ftands, I hope to have clearly demonftrated long before this, for I have plainly feen, and endeavoured to fhew mankind, that the fofter and harder parts of zoophytes are fo clofely connected with one another, that they cannot feparately exift; and therefore have not hefitated to call them conftituent parts of the fame body, and that the polype-like fuckers are fo many mouths belonging thereto.

Now, for the fmallnefs of the pores, which the Doctor has mentioned here (among the Corallines) to be a contradiction to animal life; he certainly has forgot one circumftance, when he introduces the Corallium pumilum album (Effay Cor. T. 27. *f. c.*) or his Millepora calcarea (Pall. Elench. p. 265.) as an animal, which is, that he there fays, it has abfolutely no pores at all †.

As there can be no doubt, but every part of what is called Coralline is neceffary to make out fuch an animal, or being, it will be very difficult, if not almoft impoffible, to determine the proportion there ought to be between fofter and harder parts; and therefore it cannot be thought unreafonable to fay, that in fome of this tribe the ftony parts are by much the greater part of the whole, efpecially as Doctor Pallas's objection can be only againft the cruft, or lapidefcent part, as the infide of many of them is far from being hard, being

† Pori omnino nulli. Pall. Elench. p. 266.

exactly

exactly like a Sertularia, ſo that I do not know if it would not be a good definition to one well acquainted with that tribe to ſay, a Coralline is a Sertularia covered with a ſtony or calcareous cruſt; if the mouths ſhould happen to be very ſmall, their number may make up that deficiency. We ſee in the greateſt number of Corallines their ſurface full of holes; we ſaw the ſame in Eſcharas and Milleporas thirty years ago; ſince that time magnifying glaſſes have been improved, ſo as to ſhew us, that they are all orifices, for polype-like ſuckers; why ſhould not we now admit that glaſſes may be ſtill more improved, ſo as even to make us able to ſee what may be the intention and uſe of theſe minute orifices, which according to all rules of reaſoning, we muſt ſuppoſe to approach in nature to them they are moſt alike. From this extreme minuteneſs then of the pores of theſe Millepora, confeſſed to be zoophytes, as well as thoſe of Corallina officinalis as before mentioned, it is no great matter of ſurprize, that Doctor Juſſieu could not perceive any animal life in the Corallines, nor Doctor Schloſſer in the Millepora calcarea. As theſe experiments ought to be attended with many convenient coinciding circumſtances that do not often happen to perſons who only go to the ſea-ſide, perhaps for a few days, or hours, ſo that it is unreaſonable to conclude, becauſe they have been unſucceſsful, that more accurate obſervers may not be more fortunate at another time.

I believe I ſhall be juſtified in this, by many eſſays that have been made, by perſons of judgment, to obſerve the polype-like ſuckers in many, even of the Sertulariæ, which they have ſeveral times attempted

in

in vain; I muſt own it has often happened to me in many ſpecies, and yet I have not the leaſt doubt of their being true Sertulariæ from the ſimilarity there is in their habit and form to others of the ſame genus; and of this fact I am ſure Doctor Pallas is fully convinced.

Another argument made uſe of by Doctor Pallas, to overthrow the animal exiſtence of Corallines, is taken from Mr. Meeſe's aſſertion, that he had found on Bergummer Heath in Frieſland, a ſubſtance of the ſame nature with the Corallines. Meeſe, in his Flora Friſica, p. 75. calls it a Lichen; but Doctor Pallas has ventured in his Elench. p. 427. to rank it among the Corallines, under the name of Corallina terreſtris *. In this Doctor Pallas is in the right, as I have had an opportunity of examining a ſmall ſpecimen, that my worthy and learned friend Doctor Schloſſer of Amſterdam was ſo kind to procure me: but how ſuch a nice and accurate philoſopher as Doctor Pallas could let it eſcape him to conſider the nature and quality of this ſubject, and how much it differs from any thing elſe growing on the land, is a thing that ſurpriſes me. It only being mentioned by Mr. Meeſe, as found on Bergummer Heath, ought not to have ſatisfied him ſo far, as to declare a body with a calcareous cruſt to be a land production, when no ſuch thing in the whole vegetable kingdom has ever been found; it has always been thought quite the contrary, that a ſtony or hard ſubſtance of that nature, could not be produced, but from an animal, and chiefly thoſe that live under water †.

* See the figure of it in Tab. XVII. fig. 28.

† 'Tis worthy of our notice how eaſily this ingenious Natural Hiſtorian reconciles it to himſelf, that this inhabitant of the

This

This should certainly have made him minutely inquire in what manner it was found, if buried under moss, loose on the ground, or perhaps near some of the canals, which communicate with the sea. Many accidents might have brought it thither, which is more probable than to imagine nature to go out of her usual track.

It is not improbable that that part of Holland has been overflowed by the sea, and this production left there when the water subsided, or blown there by a storm, which I beg leave to believe till I am better informed. I do not in the least doubt of Mr. Meese's veracity; but as that gentleman was more intent on discovering vegetables than animals, and thinking this very like a dry Lichen fruticulosus, he did not scruple to believe it to be one of that tribe; and therefore, perhaps, neglected to observe all those circumstances, that we now wish to be informed of.

The irregular pedunculated figures or fructifications (as Doctor Pallas pleases to call what is represented in Tab. XVII. fig. *29.*) seem to be rather a defect in the growth of the ramifications, especially as they differ from one another in shape, and some of them appear beginning to form other branches.

In fig. *a* the whole consists of two opposite curled processes, with a small cavity between them at the top; this cavity is filled up at fig. *b.* so that the top becomes rounded; in fig. *c. c.* there seems to be a beginning of a continuation lengthways; and in fig. *d.* it is still more plain the beginning of a branch.

sea can grow on dry land. See Pallas Zoophyt. p. 427. Nec magis miror Corallinam in sicco crescentem, quam Lichenum cum Fucis summam analogiam.

If

If the inside of these processes had been hollow, and the outside of a regular figure, I should not have hesitated to consider them to be the ovaries of the Coralline; but as they are solid, and of the same structure with the rest of Corallines, I shall rather call them defective branches.

Doctor Pallas's last argument to prove that Corallines are vegetables is, that the nodules, or tubercles, which he has observed in Corallines, contain little seeds subanalogous, or somewhat resembling those we find in the fructification of the Fucus's and Confervas.

If this method of reasoning should hold good, what will become of the Cellularias, Sertularias, and Millepora calcarea & agariciformis, with many other zoophytes, that have such roundish ovaries; they must be recalled to the vegetable kingdom, notwithstanding all doubt about their being living animals has long been laid aside.

I come now to his ingenious friend Doctor Baster, who carries this matter still farther, and says positively, in Phil. Transf. Vol. LII. p. 111. that the Corallines are true Confervas; and in his Opuscula Subseciva, Vol. I. Tab. I. fig. 3. A. and B. he refers us to the figure of the Corallina rubens in seed; which, he says, is a true Conferva; but the figure is so bad, that I am perswaded nobody can find out what he means to represent by it.

I shall therefore conclude this letter, with recommending to these ingenious gentlemen, to analyse these bodies chemically, and with care; and likewise to view them with the same attention, that I have done, in the microscope; if so, I am

 per-

perswaded they will be of our opinion. I must defer the sequel of what I intended to another day, which was to give you an account of the discoveries I have made in the fructification of the Confervas; these, I flatter myself, will fully convince Doctor Baster of the great difference between these two bodies, and that they belong to two different kingdoms of Nature.

I am,

SIR,

Your most obedient Servant,

Gray's-Inn, June 2, 1767.

John Ellis.

The Description of Plate XVII.

Fig. 1. The Miriozoon of Donati, or Millepora truncata of Pallas.

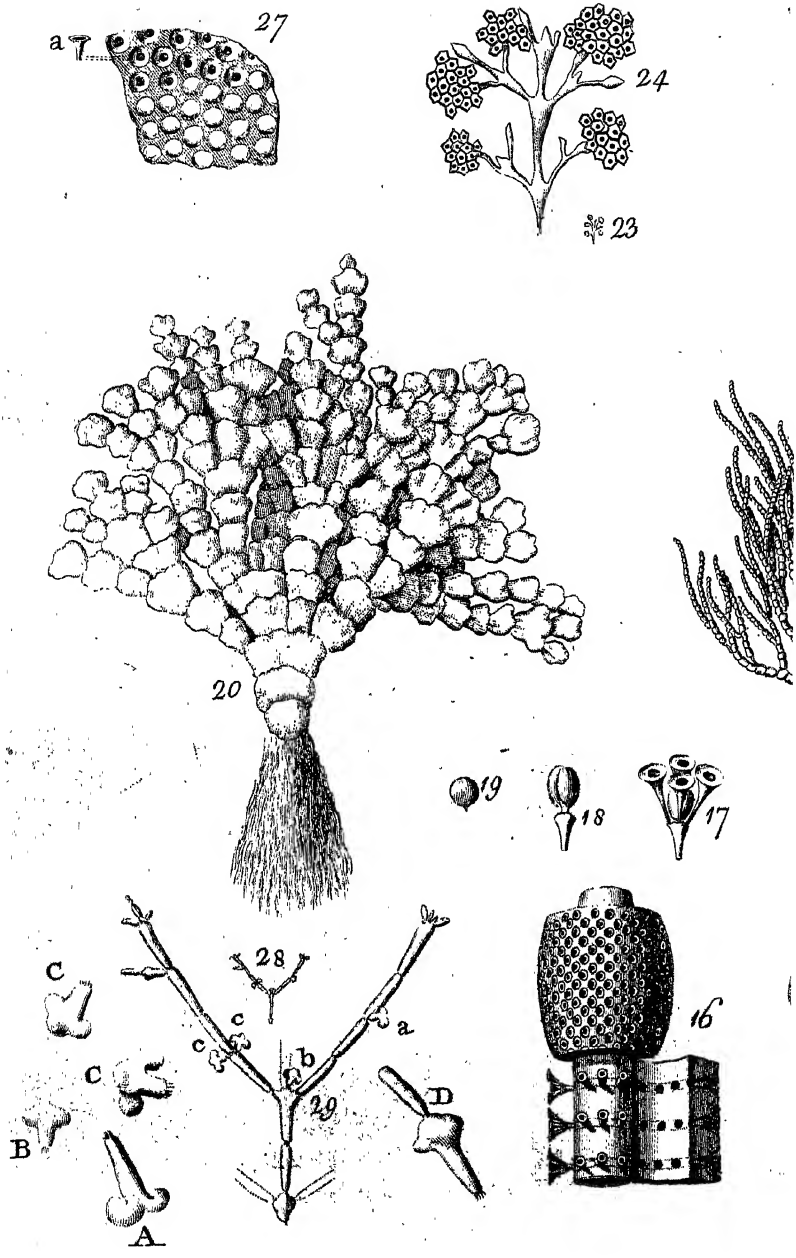
2. The end of a branch magnified, to shew the situation of the pores.

3. The same cut perpendicularly through, to shew the Trumpet-like suckers in their cells connected with the middle tubes.

4. The horizontal section of the same, with the suckers extended.

5. The magnified drawing of one of the suckers, with its cell and operculum.

6. The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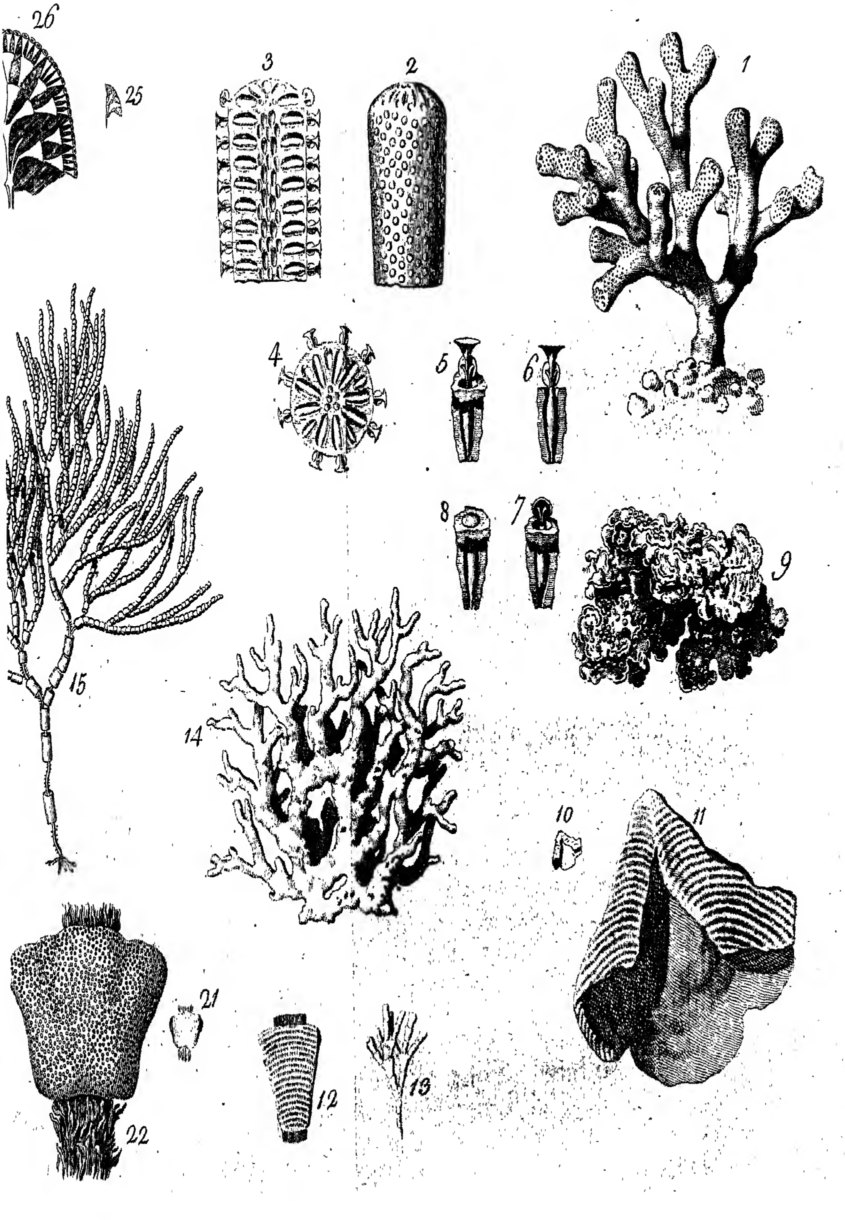
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Fig. 6. The oblique view of the opening of the cell with the ſucker and operculum.

7. The cell with the operculum open.

8. The cell covered with its operculum.

9. The Corallium Lichenoides of Ellis's Corallines, with ovaries upon it.

10. and 11. The natural and magnified ſize of a piece of this Coral, to ſhew the arrangement of the inſide of the cells, which are juſt the ſame as in the following.

12. The order of the cells, in a joint of the Corallina Officinalis, to ſhew the great affinity between them.

13. The natural ſize of a ſmall piece of the Corallina Officinalis.

14. The milk-white Millepora calcarea, from the Mediterranean, where, though the pores are not viſible on the outſide, the arrangement of the cells in the inſide are the ſame with the Corallium Lichenoides, and Corallina Officinalis.

15. The Corallina Roſarium, or White-bead band-ſtring of Sloan's Hiſt. of Jamaica, Tab. XX. fig. 3.

16. Two joints magnified, one to ſhew the ſituation and figure of the pores, and the other to ſhew how the ſuckers paſs from the middle cartilaginous tube through the calcareous covering to the ſurface.

Fig. 17. Shews four of the ſuckers, and the ovary between them, magnified highly.

18. The Ovary.

19. One of the eggs taken out of the ovary.

20. The Corallina incraſſata, from the Weſt-Indies.

21. One of the joints of its natural ſize.

22. The ſame magnified a little, to ſhew its pores in its calcareous ſurface.

23. Part of the inſide tubes of the joint, of their natural ſize.

24. The ſame magnified, to ſhew the openings of the cells on the ſurface, connected together.

25. A perpendicular ſection of half of one of theſe joints.

26. The ſame magnified, to ſhew the figure of the veſſels leading to the ſuckers in the calcareous ſurface.

27. A piece of the calcareous ſurface highly magnified, to ſhew ſome of the pores open, and others covered with their convex opercula; letter *a* ſhews the figure of one of the trumpet-ſhaped ſuckers highly magnified.

28. A ſmall branch of Meeſe's Coralline ſuppoſed to grow on a heath, called by Dr. Pallas Corallina terreſtris.

29. The ſame magnified, to ſhew the diſpoſition and figures of its ſuppoſed fructification at *a. b. c c.* and *d.* which are higher magnified at A. B. C C. and D. to ſhew how unlike they are to fructifications.

S E Q U E L.

Title-read December 17, 1767.

Read Jan. 14, 1768. I COME now to anſwer Doctor Baſter, who aſſerts poſitively, in his memoir publiſhed in the Tranſactions of the Royal Society, Vol. LII. p. 111, that all the Corallines, which you and I have deſcribed, are plants of the genus of Conferva.

In order to explain myſelf, it will be neceſſary to let him know what I mean by a Conferva, and what I would be underſtood by a Coralline, according to your ſyſtem.

By a Conferva I mean a plant with jointed filaments, either ſingle or branched, bearing fruit, which are diſpoſed in different ways: in Latin, thus,

Conferva eſt planta, cui ſunt filamenta articulata, vel ſimplicia vel ramoſa, fructificationes vario modo diſpoſitæ habentia.

By a Coralline I mean an animal growing in the form of a plant, whoſe ſtem is fixed to other bodies. The ſtem is compoſed of capillary tubes, whoſe extremities,

tremities paſs through a calcareous cruſt, and open into pores on the ſurface. The branches are often jointed, and always ſub-divided into ſmaller branches, which are either looſe and unconnected, or joined, as if they were glued together: in Latin, thus,

Corallina eſt animal creſcens habitu plantæ.

Stirps fixa, e tubis capillaribus per cruſtam calcaream poroſam ſeſe exerentibus compoſita.

Rami ſæpe articulati, ſemper ramuloſi, vel divaricati liberi, vel conglutinati et connexi.

This difference then will evidently appear by putting each kind into an acid liquor. The Coralline will immediately diſcover the nature of its * calcareous ſurface, by a ſtrong fermentation; when the Conferva will not appear in the leaſt affected. This acid liquor will likewiſe ſoon diſſolve the calcareous ſubſtance in the Coralline, by which means the minute veſſels that lead to the pores on the ſurface will become viſible; whereas the Conferva will unalterably remain the ſame, and be rather preſerved than corroded by the acid.

When Doctor Pallas, who ſupports the opinion of Doctor Baſter, comes to the chemical analyſis of the Corallines, he tells us † that he had not time nor opportunity to try them; but depends on the report of other authors.

* Lin. Syſt. Nat. Ed. 12. p. 1304. "Corallinas ad regnum animale pertinere ex ſubſtantia earum calcarea conſtat, cum omnem calcem animalium eſſe productum veriſſimum fit."

† Pallas Zooph. p. 418. "Temporis anguſtia et opportunitas impediverit ne in Corallinarum naturam accuratius igne inquirerem."

This

This dependance on the authority of others, to overturn what I think we have eſtabliſhed with very ſtrong evidence, will, I am in hopes, convince him of the propriety of that well-choſen motto of the Royal Society. "Nullius in verba;" which I find he has adopted as the common ſeal of his epiſtles to his literary correſpondents: and he will now have a further opportunity of * complimenting Doctor Baſter on making a ſecond apology for what he has advanced againſt me in the Phil. Tranſ. Vol. LII. p. 111. by ſhewing him, that they have both been miſtaken in blending two very different genera of the animal and vegetable kingdoms of nature together.

To make this difference appear ſtill more evident, I come now to lay before you a new ſcene of nature; which an accurate examination into the fructification, as well as the articulations, of ſome of the Confervas, afforded me. Indeed the minuteneſs of theſe objects would ſcarce ſeem worth while to examine into ſo critically, if my reputation had not engaged me to ſhew the wide difference between them and Corallines. This, joined to ſome remarkable diſcoveries, which I made in the year 1754 on the coaſt of Suſſex (in company with Mr. G. D. Ehret, F. R. S.) in the fructification of this claſs of plants, which

* Pallas Zooph. p. 20. "Candidiſſimus Baſterus, qui huc-"uſque contra Elliſium reliquoſque prioris ſententiæ patronos "ſteterat, alterius evidentiæ victas dedit manus, et glorioſiſſimo "exemplo, repudiata priori ſua opinione, veram theoriam acri-"ter defendere cœpit."

 before

before that time were efteemed by * botanical writers to have no fructification at all, has induced me to lay a few fpecimens of them with their magnified drawings before the Royal Society.

In examining thefe plants I was amazed to find two fpecies of them evidently of your clafs of Diœcia; that is, male parts of fructification on one, and female on the other.

The firft of thefe is the Conferva polymorpha, where in Tab. XVIII. at fig. *a.* is reprefented a very fmall branch of the female in its natural fize, and at fig. A. the fame is magnified: in the tranfparent capfules of this fpecimen, we can eafily difcover the feed as it lies expanded in a watch-glafs in water. Letter *b.* reprefents the natural fize of a fmall branch of the male. Letter B. the fame branch magnified, fhewing its amentaceous flowers, or catkins, with its minute male feed in fpikes. B 1. fhews one of them highly magnified.

The other Conferva is the Plumofa, and is one of our moft elegant fub-marine plants. Fig. *c.* reprefents the natural fize of a minute fprig of the female. At fig. C. the fame is magnified, where the feeds appear in their capfules. The fig. *d.* fhews the natural fize of a fprig of the male Conferva plumofa; and fig. D. the fame fprig magnified, fhewing the fpikes of male feed.

* Ray, Synop. Ed. 3. p. 57. "Conferva eft Mufci genus "fterile et capitulis floridis deftitutum, immo nec peltis & tu"berculis, quæ horum loco aliqui gerunt, donatum, ex meris "foliis teretibus et uniformibus feu mavis cauliculis, in tenuia "capillamenta divifis, conftans."

The

The next is the Conferva flofculofa, and is reprefented at fig. *c.* in a branch of the natural fize. Fig. E. is the fame magnified. This is one of thofe remarkable Confervas that has footftalks to its flowers or fructification. It appears to have fruit like a ftrawberry, or rafpberry, furrounded by a leafy calyx.

This was found on the fea-coaft, near Yarmouth in Norfolk, by my worthy friend George Whatley, Efquire, in the year 1764. When it was frefh, it was of a moft vivid carmine colour. The other with flowers, at fig. *f.* is the Conferva geniculata. Fig. F. fhews the fame branch more diftinctly, being magnified with flowers furrounding the joints; this, with one which I have called in my catalogue of Confervas, Conferva florifera, I difcovered in the year 1754 near Brighthelmftone in Suffex, when Mr. Ehret was fo kind as to make drawings of them while recent. The colour of this, when frefh, is a fine fcarlet.

The Conferva plumula, at fig. *g.* is one of the fmalleft of the tribe, but moft elegantly feathered; it is of a pale red colour. The fame is magnified at fig. G. which fhews the order that the fruit and branches are difpofed in. G 1 fhews the fruit or feeds, which are of a red colour, furrounded by a clear gelatinous pulp.

The Conferva at fig. *h.* I have called Ciliata, from the circle of fmall fibres at the top of each joint. The magnified drawing at fig. H. fhews thefe fibres like a crown on each joint. This was inferted here to fhew, with the reft, fome of the infinite variety of beautiful forms, which the great Author of nature has impreffed even upon one of the loweft claffes of the vegetable tribe.

Before I conclude, I must observe;

That as Doctor Pallas has likewise introduced among his arguments, that the fruit of the Fucus's are so analogous to those of the Corallines, I could introduce an infinite variety to shew the great difference there is between them; but this part of natural history, too long neglected, requires a volume by itself, to shew the amazing variety of vegetables, that lie hid from us in the great deep; I may make some observations on them the subject of a future letter, especially as many of them are of the class of Diœcia, as well as those which I have already shewn in the Conservas; which I believe will be new to the botanists.

I am,

Dear Sir,

Your most obedient servant,

John Ellis.

The Description of Plate XVIII.

Fig. *a*. The female Conserva polymorpha.

A. The same magnified, to shew the seed in the Capsules.

b. The male Conserva polymorpha.

B. The same magnified, with its male flowers.

B 1. One of the catkins, or male flowers, highly magnified.

c. The female Conserva plumosa.

The

Phil: Trans: Vol: LVII. Tab: XVIII. p. 426.

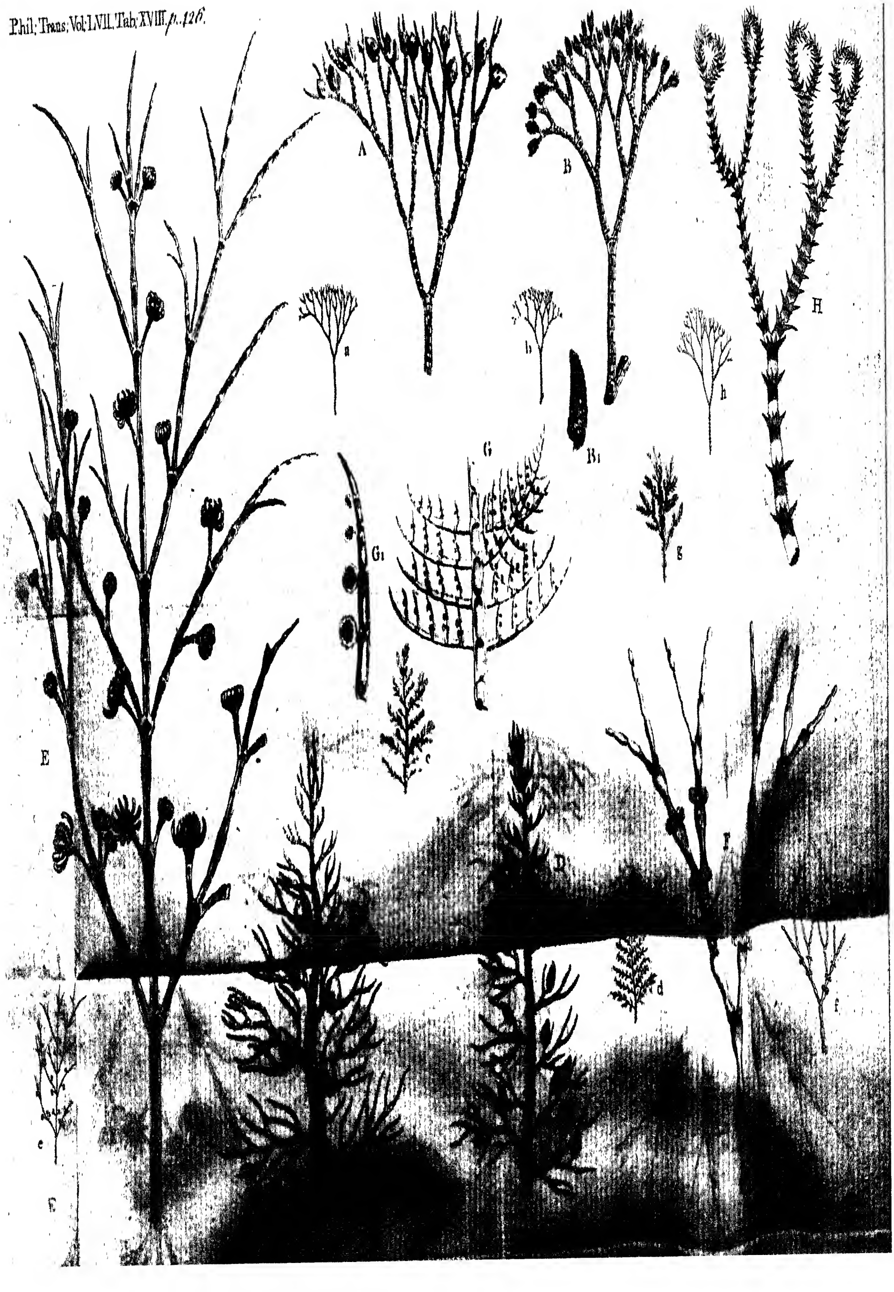


Fig. C. The fame magnified, to fhew its fructification.

d. The male Conferva plumofa.

D. The fame magnified, fhewing its catkins, or male flowers.

e. Conferva flofculofa.

E. The fame magnified, fhewing its pedunculated flowers, or fruit, with their polypetalous cups.

f. Conferva geniculata.

F. The fame magnified, to fhew its flowers furrounding the joints.

g. Conferva plumula.

G. Part of it magnified, to fhew the difpofition of its branches.

G *n.* Some of the fruit highly magnified, to fhew its feeds, furrounded by a clear vifcid pulp.

h. Conferva ciliata.

H. The fame magnified, to fhew the little coronets on the joints.

XLI. *An Account of the Actinia Sociata, or Clustered Animal-flower, lately found on the Sea-Coasts of the new-ceded Islands: In a Letter from* John Ellis, *Esquire, F. R. S. to the Right Honourable the Earl of* Hillsborough, *F. R. S.*

My Lord,

Read Nov. 12, 1767.

AMONG the many curious marine animals, which your Lordship has received from the new-ceded islands in the West-Indies, there is one most uncommonly rare: this is of great consequence to natural history, as it seems to bring together two remarkable genera in the system of nature, which Professor Linnæus had removed far from each other.

The one is the Actinia or Animal flower, the other the Hydra or Fresh-water polype.

The Actinia, called by old authors, as Aldrovandus, Johnston, &c. Urtica marina, from its supposed property of stinging, is now more properly called by some late English authors the Animal flower. This name seems well adapted to it; for the claws, or tentacles, being disposed in regular circles, and tinged with a variety of bright lively colours, very neatly represent the beautiful petals of some of our most elegantly fringed and radiated flowers, such as the Carnation, Marygold, and Anemone. As there are

are great variety of ſpecies of this animal, ſo theſe ſpecies differ from each other in their form. The bodies of ſome of them are hemiſpherical, others cylindrical, and others ſhaped like a fig. Their ſubſtance likewiſe differs; for ſome are ſtiff and gelatinous, others fleſhy and muſcular; but they are all capable of altering their ſhape, when they extend their bodies and claws in ſearch of their food. We find them on our rocky coaſts at low water, fixed in the ſhallows to ſome ſolid ſubſtance, by a broad baſe like a ſucker; but they can ſhift their ſituation, though their movement is very ſlow.

They have only one opening, which is in the center of the uppermoſt part of the animal; round this are placed rows of fleſhy claws; this opening is the mouth of the animal, and is capable of great extenſion: it is amazing to ſee what large ſhell fiſh ſome of them can ſwallow, ſuch as muſcles, crabs, &c. when it has ſucked out the fiſh, it throws back the ſhells through the ſame paſſage. Through this opening is [illegible] alive, already furniſhed, [illegible] ſoon as they fix themſelves, they begin to extend in ſearch of food.

They are found all round the coaſts of England; but the coaſts of Suſſex and Cornwall furniſh us with the greateſt varieties of them. The iſlands in the Weſt Indies are likewiſe remarkable for many kinds of them, as appear from the different ſorts ſent to your Lordſhip by Mr. Greg.

Doctor Gaertner, F. R. S. who has deſcribed four ſpecies of the Engliſh ones in the Phil. Tranſ. *, ſays

* Vide Phil. Tranſ. Vol. LII. p. 75. Tab. I. fig. [illegible] 4, and 5; the animal in fig. 3, in the ſame Plate is marked in

I they

they have the remarkable property of renewing their claws when they are cut off; and ranks them, perhaps very properly, under the genus of Hydra of Linnæus, or Fresh-water polype: which I shall now give a short description of, that we may judge how near your Lordship's new animal approaches to both of these.

The Hydra, or Fresh-water polype, is that extraordinary animal so well known to the curious, from the discoveries of Mr. Abraham Trembley, F. R. S. in its re-production after it had been cut into pieces. When it is extended, it is of a worm-shaped figure, and of the same tender substance with the horns of a common snail.

It adheres by one end like a sucker to water plants and other substances: the other end, which is the head, is surrounded by many arms or feelers placed like rays round a center: this center is its mouth, and with these arms, which are capable of great extension, it seizes small worms and water insects, and brings them to its mouth; often swallowing bodies larger than itself: when the food is digested in the stomach, it returns the remains of the animals it feeds on, through its mouth again, having no other visible passage from its body.

Their manner of multiplying is from eggs, which they produce in autumn *; but the most common is from their sides, in which there first appear small knobs, or papillæ; as these increase in length, little

this genus by Doctor Pallas, as well as Doctor Gaertner, but very improperly, as it has many feet, and a passage through its body. Doctor Linnæus calls it *Holothuria*.

* See Pallas, Zoophyt. p. 28.

fibres

fibres are ſeen riſing out of the circumference of their heads, which they ſoon uſe to procure food. When they are thus arrived at a mature ſtate, they ſend forth other young ones from their ſides: ſo that though many of them ſoon fall off, and provide for themſelves, yet the animal frequently branches out into a numerous offspring, growing out of one common parent, each of which not only procures nouriſhment for itſelf, but for the whole family.

I come now to your Lordſhip's new animal; and, for the Satisfaction of the Royal Society, lay before them one of your Lordſhip's ſpecimens preſerved in ſpirits, with a diſſection of one of them, to ſhew its internal ſtructure, together with three ſpecies of Actinia, or Animal flowers, ſent to your Lordſhip from the new-ceded iſlands.

This compound animal, which is of a tender fleſhy ſubſtance, conſiſts of many tubular bodies, ſwelling gently towards the upper part, and ending like a bulb, or very ſmall onion; on the top of each is its mouth, ſurrounded by one or two rows of tentacles, or claws, which when contracted look like circles of beads.

The lower part of all theſe bodies have a communication with a firm fleſhy wrinkled tube, which ſticks faſt to the rocks, and ſends forth other fleſhy tubes, which creep along them in various directions. Theſe are full of different ſizes of theſe remarkable animals, which riſe up irregularly in groupes near to one another.

This adhering tube, that ſecures them faſt to the rock, or ſhelly bottom, is worthy of our notice. The knobs that we obſerve, are formed in ſeveral

parts

parts of it, by its infinuating itfelf into the inequalities of the coral rock, or by grafping pieces of fhells, part of which ftill remain in it, with the flefhy fubftance grown over them.

This fhews us the inftinct of nature, that directs thefe animals to preferve themfelves from the violence of the waves, not unlike the anchoring of mufcles, by their fine filken filaments, that end in fuckers; or rather like the fhelly bafes of the Serpula, or Worm-fhell, the Tree Oyfter, and the Slipper Barnicle, &c. whofe bafes conform to the fhape of whatever fubftance they fix themfelves to, grafping it faft with their teftaceous claws, to withftand the fury of a ftorm.

When we view the infide of this animal diffected lengthways, we find a little tube like a gullet leading from the mouth to the ftomach, from whence there rife eight wrinkled fmall guts, in a circular order, with a yellowifh foft fubftance in them; thefe bend over in the form of arches towards the lower part of the bulb, from whence they may be traced downwards, to the narrow part of the upright tube, till they come to the flefhy adhering tube, where fome of them may be perceived entering into a papilla, or the beginning of an animal of the like kind, moft probably to convey it nourifhment, till it is provided with claws: the remaining part of thefe flender guts are continued on in the flefhy tube, without doubt for the fame purpofe of producing and fupporting more young ones from the fame common parent.

The many longitudinal fibres, that we difcover lying parallel to each other, on the infide of the femi-

ſemi-tranſparent ſkin, are all inſerted in the ſeveral claws round the animal's mouth, and are plainly the tendons of the muſcles, for moving and directing the claws, at the will of the animal; theſe may be likewiſe traced down to the adhering tube.

As this ſpecimen has been preſerved in ſpirits, the colour of the animal when living cannot certainly be known; it is at preſent of a pale yellowiſh brown.

With regard to its name, it may be called Actinia ſociata, or the Cluſter animal flower.

Among the critics, my Lord, I am aware of this; that it may be ſaid, that an animal compounded of many animals has not a very philoſophical ſound. But it is well known to thoſe, who underſtand the nature of zoophytes; that there are many kinds of theſe animals, as well ſuch as ſwim about freely, as ſuch as are fixt to rocks and ſhells in the ſea, that have a great many mouths in the form of polypes, and yet are but ſingle animals; ſuch as the great variety of Pennatulas, or Sea pens, among thoſe that ſwim about, and moſt of the Sertularias, Gorgonias, with many others, among thoſe that are fixt. Yet this new animal of your Lordſhip's differs very much from the generality of theſe. I think I may compare it, to ſpeak in the ſtyle of thoſe who maintain that zoophytes vegetate, to a timber tree, that ſends out at a diſtance round it many ſuckers from its roots, which ſuckers coming in time to be trees, theſe may and will, with propriety, be reckoned ſo many diſtinct trees, though connected at their roots with the parent tree, and that without any abſurdity.

 Leſt

Leſt any doubt ſhould ſtill ariſe in this abſtruſe part of the operations of nature, it may be proper that I ſhould explain myſelf further, by ſhewing that there are a great many zoophytes, which were formerly called Corallines, now Sertularias and Cellularias, that from a creeping adhering tube ſend up ſeveral ſingle animals, others ſend up ſeveral branched animals. To give an inſtance or two of each, I ſhall mention the Sertularia uniflora, or Single bell-ſhaped Coralline (ſee the Eſſay on Corallines, Pl. XIV. fig. A and B) and the Cellularia anguina, or Snake's head coralline (ſee the ſame Eſſay, Pl. XXII. fig. C) both which, like our Actinia ſociata, ſend up diſtinct animals with one mouth each.

Whereas the Sertularia pumila, or Sea oak coralline (ſee Eſſay on Coralline, Pl. V. fig. A) and the Cellularia burſaria, or Shepherd purſe coralline (ſee the ſame Eſſay, Pl. XX. fig. A) ſend out animals, in the form of ſpikes or branches, that have many mouths from their own creeping and adhering tubes; and yet both thoſe with one mouth to each, and theſe with many, I eſteem as ſo many diſtinct animals, notwithſtanding their being connected by an adhering tube, as I have ſaid in the inſtance of the tree and its ſuckers.

To conclude, my Lord, the importance of the diſcovery of this new animal to natural hiſtory is this, that it clears up that much-diſputed point, which is, that the extenſion or increaſe of the ſubſtance of theſe zoophytes is of an animal, and not of a vegetable growth (as ſome late authors would have us think) by thus making the fact more clear and evident to our ſenſes.

For the poetical deſcriptions of ſome late ſyſtematical authors have tended rather to confuſe than explain theſe matters to our ideas; for inſtance, they call theſe bodies, that riſe up like a ſpike with many mouths, a vegetating ſtem, and their mouths, which are formed like ſo many polypes, flowers; though with theſe ſuppoſed flowers, they evidently ſeize their food, by ſtretching out their claws (which they call the petals) to convey it to their mouths, that are in the center of each, to ſwallow it, digeſt it, and return the non-nutritive parts back again by the ſame way. Can this then be called a vegetative life?

But happily this animal of your Lordſhip's is large enough for diſſection; and in that ſtate diſcovers to us, not only muſcles and tendons, but a ſtomach to digeſt, and inteſtines to ſecrete, proper nouriſhment for the ſupport and increaſe of itſelf and its progeny; which I am perſwaded is the ſtrongeſt proof that has yet appeared to convince the learned world, that zoophytes are true animals, and in no part vegetable; and that the Royal Society are highly obliged to your Lordſhip for this moſt valuable acquiſition in natural hiſtory, as well as he who has the honour to be,

My Lord,

Your Lordſhip's moſt devoted,

and much obliged humble ſervant

Gray's-Inn,
Aug. 17, 1767.

John Ellis.

The Defcription of Plate XIX.

Fig. 1. The Actinia fociata, or Cluftered animal flower, with its radical tube adhering to a rock. (*a*) One of the animals ftretching out its claws.

2. A perpendicular diffection of one of thefe bodies, to fhew the gullet, inteftines, ftomach, and fibres, or tendons, that move the claws. (*a*) A young one arifing out of the adhering tube.

3. The Actinia after, or Sea ftar flower, from the new ceded Iflands.

4. The Actinia anemone, or Sea anemone, from the fame place.

5. The under part of the fame, by which it adheres to rocks.

6. The Actinia helianthus, or Sea fun-flower, from the fame place.

7. The under part of the fame.

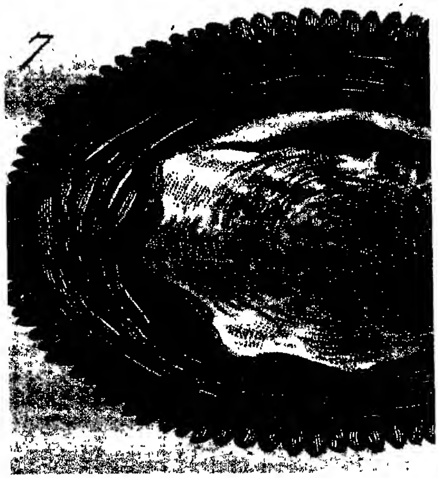
8. The Actinia dianthus, or Sea carnation, from the rocks at Haftings in Suffex: this animal adheres by its tail, or fucker, to the under part of the projecting rocks, oppofite to the town; and, when the tide is out, has the appearance of a long white fig: this is the form of it when it is put into a glafs of fea-water. It is introduced here as a new variety of this animal, not yet defcribed.

9. The

3

5

7



Phil: Trans: Vol. LVII. Tab. XIX. p 436.

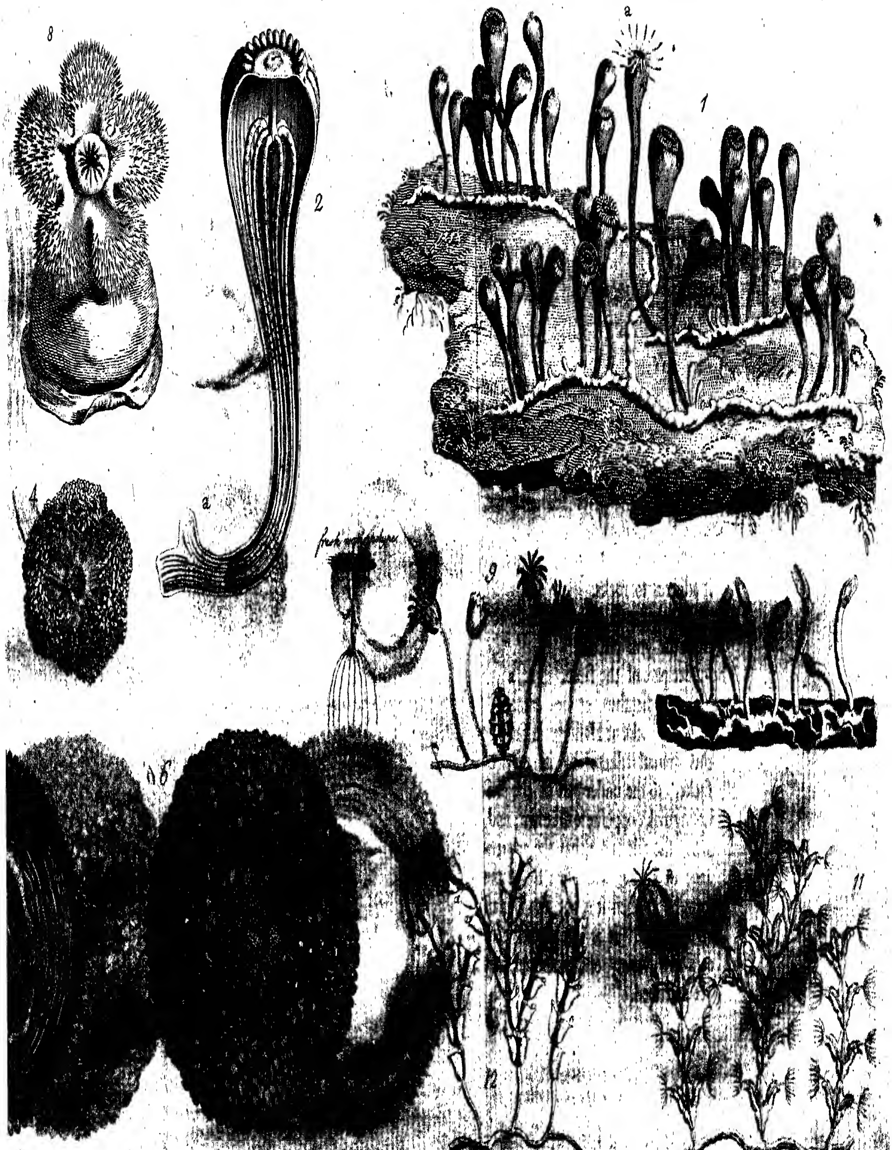


Fig. 9. The Sertularia uniflora, or Single bell-ſhaped coralline magnified. (*a*) One of its ovaries.

10. The Cellularia anguina, or Snake's-head coralline, magnified.

11. The Sertularia pumila, or Sea-oak coralline, magnified. (*a*) One of its ovaries.

12. The Cellularia burſaria, or Shepherd's purſe coralline, magnified.

Received October 10, 1767.

XLII. *A Letter from* Edward Wortley Montagu, *Esquire, F. R. S. to* William Watson, *M. D. F. R. S. containing some new Observations on what is called* Pompey's Pillar, *in* Egypt.

S I R,

Read Nov. 19, 1767.

I HERE send you a few lines, which I believe will appear extraordinary, as every traveller that has been at Alexandria has mentioned the famous pillar of Oriental Granite, which is about a mile without the walls of that city, as erected, either by Pompey, or to the honour of Pompey. As I differ in opinion from them all, and think this famous pillar was erected to the honour of Vespasian, you certainly will expect to hear on what foundation I found so extraordinary a conjecture, as so new a one may appear to you.

By

	F.	In.	
By my menf. the capital of the pillar is	9	7	British Meafure.
The Shaft ——— ———	66	1 $\frac{3}{4}$	
The Bafe ——— ———	5	9 $\frac{3}{4}$	
The Pedeftal ——— ———	10	5 $\frac{2}{4}$	
Height from the Ground ———	92	0	
Its Diameter ——— ———	9	1	

As foon as I faw this furprizing pillar, I was convinced that, if it had been erected in Pompey's time, Strabo, or fome of the ancients, would have mentioned it: I therefore determined to examine it narrowly. I perceived too that the pedeftal was of a bad and weak mafonry, compofed of fmall and great ftones of different forts, and abfolutely unable to fuftain fo great a weight; I therefore eafily concluded fuch pedeftal not originally belonging to the pillar. I attempted to get out a ftone, which I did without trouble, and difcovered the pedeftal to be hollow. After fome time, I mean during the courfe of many days, I made an opening wide enough to enter it; when within it, you will judge how much I was furprized to find this prodigious mafs of granite, ftood, as on a pivot, on a reverfed obelifk, as I then believed it was, only five feet fquare; curious to know the length of the obelifk, I began to move the earth on one of its fides, but my furprize increafed much when I found, after moving a few inches of the foil, that the obelifk was not entire, this pivot being only four feet and one inch thick. It is feated on a rock; the ftone is of an extreme hardnefs,

nefs, and almoft a petrification or rather conglutination of many different ftones, but all vitrefcent. I never met with any ftone of this kind any where, except with one fmall piece on the plain of the Mommies; I broke a piece of it, which Lord Bute has; a fmall piece too of the pillar was fent, that gentlemen may be convinced it is of red granite, and not a compofition as fome have imagined.

This part of obelifk is covered with hieroglyphicks, which are reverfed, a plain proof the pillar was not erected whilft they were held facred characters.

Convinced, therefore, that it was not of the antiquity one would fuppofe it, from being called of Pompey, I vifited it feveral times to fee if it might not be poffible to find out fomething that would give room for a reafonable conjecture, in honour of whom, or at what time, it was erected. From the infcription I could difcover nothing; it is on the weft face of the bafe, but fo much injured by time, and I may fay too by malice, for the mark of an inftrument are plainly difcovered effacing it, that one can but imperfectly make out fome Greek characters, fo imperfectly indeed that no one word can be found.

At length, obferving that the cement, or mortar, which clofes the fmall feparation of the fhaft from the bafe, was quite deftroyed in one part, I was curious to fee if any thing was made ufe of within, to faften or tie the fhaft to the bafe; I faw there was: being defirous to know if it was lead, and if fo, if it was not of that pure, and of which we ftill meet with fome few medals; I endeavoured with a pretty large hanger to cut off a fmall piece of the

grapple;

grapple; there was a great number of lizards which had taken ſhelter there, and which run out on my introducing the hanger. I then diſcovered a dark ſpot, at the diſtance of more than a foot, within the circumference of the pillar; which, by ſtriking it with the hanger, I found was ſomething ſtuck faſt to the baſe; after ſtriking it ſeveral times, I detached it from its place, and it proved a medal of Veſpaſian in fine order. ΑΥΤ. ΚΑΙΣ. ΣΕΒΑ. ΟΥΕΣΠ.... The reverſe is, *Victoria gradiens; Dextra ſpicas, ſiniſ. palmam.*

This medal was ſhewn to the Royal Society.

The reverſed hieroglyphicks are a proof that this amazing monument was not erected before Pompey's time; and as there is no mention of it in Strabo, or any one of the antient writers that I have met with, it ſeems plain it was not known before the time of Veſpaſian. This medal could not by any accident, I think, have been introduced above a foot within the circumference of the ſhaft; therefore I ſuppoſe it was placed there when the pillar was erected, which from thence I conclude to have been done to the honour of that emperor; and perhaps on his reſtoring the cripple to the uſe of his limbs.

If you think this paper worth it, you will pleaſe to communicate it to the Royal Society, and that of the Antiquaries.

The pillar is exactly ſhewn, with the pivot it ſtands upon, with a reference to the ſpot the medal was found upon, in the view of it that I have ſent to England.

 I beg

I beg you will affure the Society of my refpect, and how happy I fhall be to execute any of their commands.

And I hope you will reft perfuaded of the true confideration, with which

I am,

Dear Sir,

Your moft humble fervant,

Zante, May 7, 1767.

Ed. Wortley Montagu.

XLIII. *Part of a Letter from* W. Watſon, *M. D. F. R. S. to* John Huxham, *M. D. F. R. S. at* Plymouth, *giving ſome Account of the late Cold Weather. Dated* London, 14 February, 1767.

Dear Sir,

Read Nov. 19, 1767.

THIS waits upon you to inform you of the degree of cold we have lately experienced. After as mild a winter as has been known here for many years, the froſt has been intenſe. Until the latter end of December, many of the tender annual exotic plants continued alive; ſuch as the African Marygold, Naſturtium Indicum, and others of this claſs. I ſaw even the plant, uſually called Balm of Gilead, at that time flouriſhing without ſhelter. Mr. Miller of Chelſea, with whom I talked upon this ſubject, informed me, that he had known the like but twice in his life; and that was in the years 1717 and 1722. However, at the beginning of laſt month, January, after ſome ſmart guſts of wind at Eaſt, it began to freeze; and continued increaſing, until the fifth of that month, in the morning, my thermometer, an excellent one made by Mr. Bird, ſtood in the open air ſomewhat under 20″, in the evening it was 29″. It continued thereabouts to the ninth, when in the morning it ſtood at 20″ again, and at night at 21″. On the tenth in the morning it ſtood at 17″, at night at 18″. From this time to the eighteenth it was never below 23″, but

frequently

frequently above the freezing point. On the eighteenth at night it ſtood at 19″ and ½. On the nineteenth at ſix in the morning it ſtood at 16″, at eight in the morning 15″ ½, at eleven at night 17″. On the twentieth at eight in the morning it ſtood at 18,″ at eleven at night 22″. On the next day, January twenty-firſt, the froſt broke; the thermometer at four in the afternoon ſtanding at 36″.

In the country it has been obſerved much colder. On January tenth, at Cardington in Bedfordſhire, Mr. Howard, Fellow of the Royal Society, by two thermometers, obſerved the mercury, at ſix in the evening, to ſtand at 9″. And upon the ſame day the Rev. Mr. Wollaſton, at Eaſt Dereham in Norfolk, found it ſo low as 8″. Mr. Ellinet, at Norwich, on the nineteenth of January, found his thermometer, at eight o'clock in the morning, to ſtand at 7″; at noon at 22″; at five in the afternoon 18″; at ten at night at 8″. Seven degrees therefore is the loweſt point, at which I have heard the thermometer has ſtood any where in England during the late froſt.

January tenth was the coldeſt day at Plymouth, where Dr. Farr, a very ingenious phyſician, and Mr. Mudge, who has communicated ſeveral papers to the Royal Society, each obſerved his thermometer to ſtand at 23″ and ½.

The ſevere froſt of 1739, of which no account appears in the Philoſophical Tranſactions, began December twenty-ninth, when Lord Charles Cavendiſh's thermometer in his room ſtood at 25″; the next evening 21″.

From this time his Lordſhip placed his inſtrument out of the window, and at ſome diſtance from it; when

in the evening of December thirtieth, it ſtood at 17″. On the morning of January fifth, it ſtood at 13″. On the eighth at 15″. The ninth at 14″. Twenty-ſecond at 19″. Twenty-fifth at 17″. On February fifth at 21″. During all this time, the thermometer was ſeldom above 32″, the freezing point.

Thirteen degrees, therefore, during this froſt, was the loweſt obſervation, by Lord Charles Cavendiſh in Marlborough Street.

I am,

with all poſſible reſpect,

Dear Sir,

your moſt obliged,

and obedient Servant,

W. Watſon.

Abstract from a Meteorological Diary kept at Plymouth.

This contains Obſervations twice a Day; the Barometer and Thermometer are made by Siſſon, the Rain Machines alſo, and ſimilar to that deſcribed in the Edinburgh Medical Eſſays.

N. B. One of the Thermometers is kept in a Parlour where there is no Fire, the other without Doors in the Rain Machine, which is never affected by the Sun, but was not examined before or after the Sun ſet; ſo that the loweſt State of the Thermometer in theſe Parts is not aſcertained.

JANUARY 1767.

Day		Bar.	Ther. within doors.	Ther. without doors.	Wind.		Rain	Weather.
1	9 a. m.	30 20	44		N.	2		Clear
	11 p. m.	29 84	45		E. b. N.	3		D° a remark. guſt of wind for ſome h. in the n.
2	9 a. m	29 82	42	32	N. E.	2‴		Clear
	11 p. m.	29 65	40		N. E.	2‴		D° with froſt this night and the laſt
3	9 a. m.	29 66	40	30	N. N. E.	2‴		Fair; a few flakes of ſnow fell thro' the day
	11 p. m.	29 77	39		N. E.	2		Clear
4	9 a. m.	29 88	38	30	E.	2′		D° a very ſharp air
	11 p. m.	29 96	35		E.	2		D°
5	9 a. m.	19 95	33		N. E.	2		D°
	11 p. m.	29 91	35		N. W.	2		D°
6	9 a. m.	29 75	35		N. N. W.	2		D°
	11 p. m.	29 74	38	33	N. W.	2		Cloudy; little or no froſt
7	9 a. m.	29 64	39		E. b. N.	0		Hazy
	11 p. m	29 56	39		S. W.	1	.230	Rain, heavy; but towards morn. ſevere froſt
8	9 a. m.	29 61	36		E. b. N.	2		Froſt; clear with a very ſharp air
	11 p. m.	29 70	35		N. E.	2		Heavy falls of ſnow
9	9 a. m.	29 70	35		N.	1		Slight falls of ſnow at times
	11 p. m.	29 72	35		N. W.	2		Clear and ſharp air
10	9 a. m.	29 60	34	23 5	N. W.	2		D°
	11 p. m.	29 46	34		S. W.	2‴		Cloudy; and towards morning rain

Jan. Day		Bar.	Ther. within doors.	Ther. without doors.	Wind		Rain	Weather
11	9 a. m.	29 22	33	24	N.	1	.110	Hail; fleet; and after, moft heavy fall of fnow
	11 p. m.	29 20	33		N.	1		Cloudy; fnow in 3 h. cov. upw. 6 inch. deep
12	9 a. m.	29 17	32	24	N. W.	1		Clear wind with the fun; a very fine day
	11 p. m.	29 15	35		S. W.	2		Cloudy. N. B. Thermometer at noon 24
13	9 a. m.	28 93	37		S. S. E.	2‴		Cloudy; fnow every where melt. by the thaw
	11 p. m.	28 67	39		S. E.	3		Rain heavy through the night; ftormy
14	9 a. m.	28 67	40		S.	2	.960	Heavy rain in the morn. fhowery thro' the day
	11 p. m.	28 90	42		S.	2		Showery
15	9 a. m.	29 30	44		S. S. E.	2″		Cloudy, with fun-fhine at times
	11 p. m.	29 36	45		S. S. E.	2‴		Cloudy
16	9 a. m.	29 42	44		S. S. E.	2‴	.320	Showery
	11 p. m.	29 55	44		E.	2		Cloudy
17	9 a. m.	29 70	40		N. E.	2		Cloudy; with a fharp cold air
	11 p. m.	29 80	44		E. b. N.	2		D°
18	9 a. m.	29 82	36		N. E.	2		Fair; air very fharp
	11 p. m.	29 90	35		N. E.	2		Clear and froft
19	9 a. m.	29 90	35		N. N. W.	1	.070	Clear till 4 p. m. then fhowery
	11 p. m.	29 92	38		N.	0		Clear and froft
20	9 a. m.	30 0	36	27	N.	0		D°
	11 p. m.	30 8	38	34	W.	1		Cloudy; thaw; towards morning rain

Depth

Depth of Rain which has fallen at Plymouth during the Year 1766.

1766	Inc.	Dec. Parts	Barom. hig. ſtate		Barom. low. ſtate	
January	0	.165	30	74	29	83
February	2	.590	30	65	29	40
March	1	.690	30	45	28	83
April	3	.780	30	43	29	26
May	2	.600	30	23	29	21
June	3	.880	30	21	29	33
July	3	.550	30	3	29	56
Auguſt	1	.600	30	10	29	44
September	2	.630	30	20	29	45
October	2	.650	30	45	29	08
November	4	.660	30	50	28	62
December	3	.280	30	44	29	14
Inches	35	.075				

N. B. In the latter end of December 1766, the weather was ſo mild, and had been ſo in the preceding weeks, that ripe ſtrawberries were gathered in my garden at the Royal Hoſpital.

Meteorogical

Meteorological Register, kept at the Royal Hospital near Plymouth, continued from January 20, to March 1, 1767.

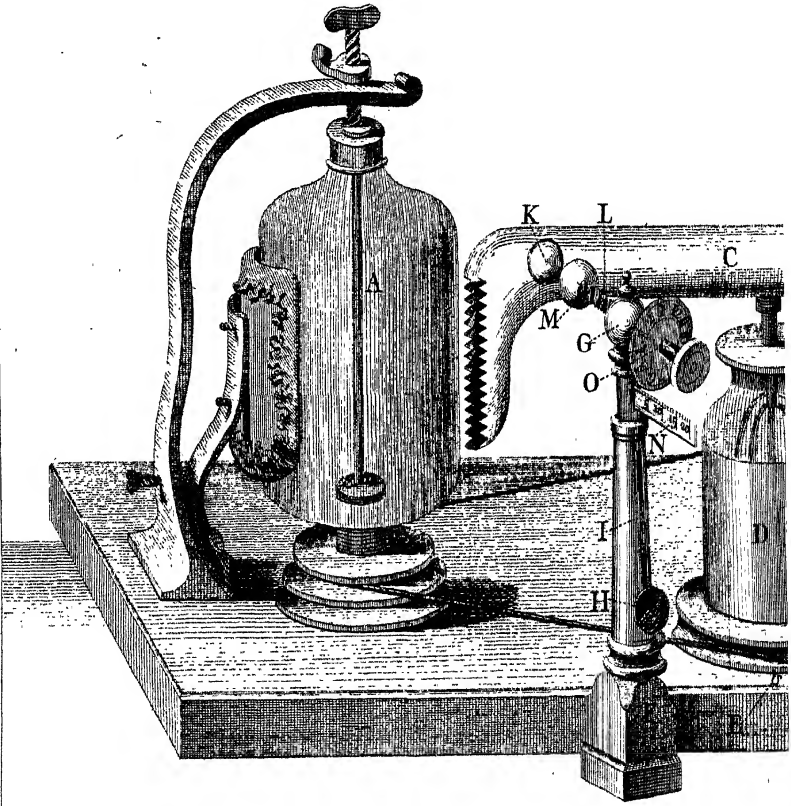
Day Jan.		Bar.	Thr.	Wind		Rain	Weather	Remarks
21	9 a. m.	29 97	39	W. b. S.	2		Slight showers	
	11 p. m.	29 92	40	W. b. S.	1		Cloudy	
22	9 a. m.	29 67	42	W. b. S.	2″		Small rain constant thro' the day	
	11 p. m.	29 74	44	N. W.	2		Cloudy	
23	9 a. m.	29 94	44	N.	2		Showery, but clear for the most part	
	11 p. m.	29 0	45	N.	2		Cloudy weather	Highest state of Barom. } 30 5
24	9 a. m.	29 90	46	S. W.	2		Small rain through the day	
	11 p. m.	29 81	48	S. W.	2′	.310	D°	Lowest state of Barom. } 29 4
25	9 a. m.	29 72	49	S. W.	2‴		Cloudy, with small rain at times	
	11 p. m.	29 65	49	S. W.	2‴		D°	
26	9 a. m.	29 62	49	S. W.	2‴		Showers in the forenoon, afterw. clear	Highest state of Therm. } 53
	11 p. m.	29 68	49	W. b. S.	2	.148	Cloudy	Lowest state of Therm. } 39
27	9 a. m.	29 50	50	S.	3		Rain heavy and constant thro' the day	
	11 p. m.	29 58	50	S. S. E.	2″		D°	The quantity of rain
28	9 a. m.	29 67	50	N. N. E.	2	.960	Small rain through the day	is very considerable during this period;
	11 p. m.	29 50	49.5	N. E.	1		Rain	and yet, on looking back into the re-
29	9 a. m.	29 77	46	S. S. E.	2	.572	Rain from noon till 4 p. m.	gister, I find that in
	11 p. m.	29 77	48	S. S. E.	2		Cloudy	the month of March
30	9 a. m.	29 73	50	S. W.	2		Fair till 11 a. m.	1765,
	11 p. m.	29 72	51	S. W.	2	.530	Rain constant since 11 a. m.	Inc. the total is 10.820
31	9 a. m.	29 73	52	S. S. W.	2		Rain constant, with hazy weather	and taking in from the 23d of February,
	11 p. m.	29 78	53	S.	2	420	Small rain	to the 2d of April
Feb.								1765, viz. 39 days,
1	9 a. m.	29 90	52	S. S. E.	1		Fog, with small r. aft. heavy thro' day	Inc.
	11 p. m.	29 74	53	S. S. W.	2		Cloudy	the amount is } 13.900
2	9 a. m.	29 09	52	S. E.	1		Hazy weather, with rain at times	
	11 p. m.	29 91	53	S. E.	1	.240	D°	A quantity equal,
3	9 a. m.	29 83	51	S. E.	2		Fair, with sun-shine thro' the day	I suppose, to what
	11 p. m.	29 72	52	E.	1		Clear	usually falls in five
4	9 a. m.	29 60	51	E.	2		Fair through the day	months, in most parts of England,
	11 p. m.	29 56	49	E. b. S.	2		Clear	being nearly fourteen
5	9 a. m.	29 54	51	E.	2		D°	inches.
	11 p. m.	29 59	50	E.	2′		Cloudy	
6	9 a. m.	29 60	48	E. b. S.	2		Small rain, with hazy weather	
	11 p. m.	29 41	51	E.	2″		Rain	
7	9 a. m.	29 23	51	E.	2	.340	Hazy, with showers	
	11 p. m.	29 15	51	E.	2		Heavy showers, with squalls.	

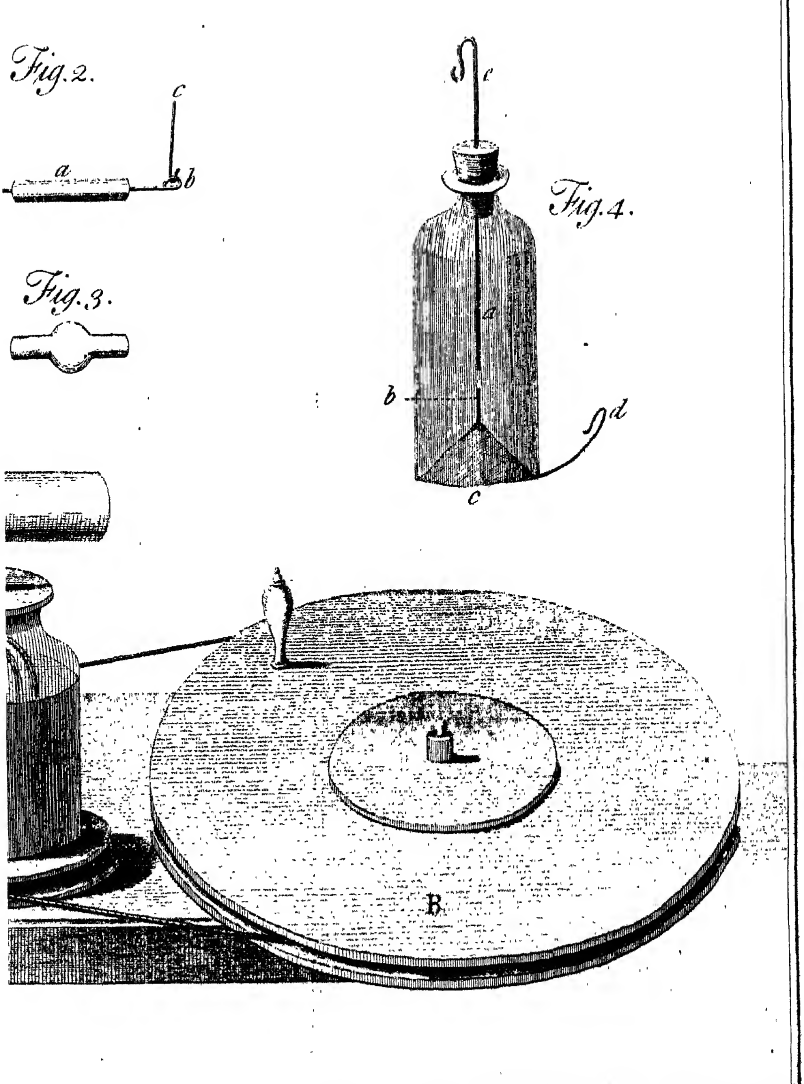
Day Feb.		Bar.	Thr.	Wind		Rain	Weather
8	9 a. m.	29 15	51	S. W.	2″	.600	Hazy ſhowers through the day
	11 p. m.	29 13	50.5	S. W.	2		Cloudy
9	9 a. m.	29 32	51	E.	1		Hazy weather, with rain till 1 p. m.
	11 p. m.	29 37	51	S. W.	2		Cloudy, but afternoon fair
10	9 a. m.	29 17	51	S. W.	2″		Thunder ſhowers heavy, with ſqualls
	11 p. m.	29 48	51	W. b. W.	2		Showery
11	9 a. m.	29 23	51	S. E.	2	.770	Rain conſt. from 11 a. m. thro' the d.
	11 p. m.	29 20	51	S. S. W.	3		Rain, with very heavy ſqualls
12	9 a. m.	29 35	50	N.	2		Slight ſhowers, with ſun-ſhine between
	11 p. m.	29 52	49	N.	1		Clear and ſerene
13	9 a. m.	29 2[illegible]	49	S. E.	2‴		Rain conſtant; hazy, w. var. to S.W.
	11 p. m.	29 17	50	W.	2‴	.990	Heavy ſhowers
14	9 a. m.	29 34	50	W.	2″		Hail ſtorms, with heavy ſhowers
	11 p. m.	29 50	51	S. W.	2		Cloudy
15	9 a. m.	29 54	51	S.	2	.670	Rain very heavy till the afternoon
	11 p. m.	29 57	51	W. b. S.	2		Clear
16	9 a. m.	29 53	50	S. S. W.	2′		Cloudy, with ſmall rain at times
	11 p. m.	29 63	50	S. W.	2		Cloudy weather
17	9 a. m.	29 28	51	S.	2	.450	Rain conſtant till 2 p. m.
	11 p. m.	29 32	50	S. W.	2		Showery
18	9 a. m.	29 38	51	S. W.	2		Rain till noon
	11 p. m.	29 67	51	S. W.	2	.420	Showery; but clear for the moſt part
19	9 a. m.	29 67	51	S. W.	2″		Clear
	11 p. m.	29 68	51	S. S. W.	3		Rain in the night very heavy
20	9 a. m.	29 26	51	S. E.	2‴	.710	Rain conſtant through the day
	11 p. m.	29 4	51	S. W.	3		Hail-ſt. with moſt heavy ſqualls of w.
21	9 a. m.	29 32	51	S. E.	2	.300	Fair, with ſun-ſhine
	11 p. m.	29 14	49	S.	2		Rain ſince 5 p. m.
22	9 a. m.	29 43	48	W. b. N.	2		Fair
	11 p. m.	29 65	48	W.	2		Showers in the even. at preſent clear
23	9 a. m.	29 80	48	S.	1		Clear
	11 p. m.	29 87	49	W. b. N.	2	.716	Cloudy; in the night, heavy rain
24	9 a. m.	29 86	50	W.	2′		Rain drizling through the forenoon
	11 p. m.	36 5	50	W. b. N.	2		Cloudy
25	9 a. m.	29 74	51	S. W.	2″	.510	Rain conſtant till 3 p. m.
	11 p. m.	29 70	51	W.	2		Showery
26	9 a. m.	29 51	51	S. W.	2‴		Rain drizling conſtant through the day
	11 p. m.	29 44	51	S. W.	3		Rain; wind at S. W. a ſtorm
27	9 a. m.	29 5[illegible]	52	N. W.	2″		Black and cloudy weather; but little r.
	11 p. m.	29 56	51	N. W.	2″		D°
28	9 a. m.	29 6[illegible]	49	N. N. W.	2″	.284	Showery in the morn. afterw. Hail-ſt.
	11 p. m.	29 90	19	N. N. W.	2″		Show. at times; but fair for the moſt part
				Total	94	.940	or near 10 inches of rain in 39 days

Remarks

N. B. The machine is placed free of eddy winds, at a very confiderable diftance from any building, except to the North, where, however, it is fixty feet diftant, and from that quarter we have little or no rain.

Fig. 1.





J. Mynde sculp.

Received August, 1767.

XLIV. *Description of an Electrometer invented by Mr.* Lane; *with an Account of some Experiments made by him with it: In a Letter to* Benjamin Franklin, *LL. D. F. R. S.*

Aldersgate-Street, October 15, 1766.

S I R,

Read Nov. 26, 1767.

BEING employed in some electrical enquiries about the beginning of the year 1762, it occurred to me, that many experiments on this subject might be made with a much greater degree of precision, if we could determine, with any tolerable accuracy, the comparative quantity of electric fluid, with which, for any given experiment, the coated phial is impregnated.

An instrument, which I have contrived for this purpose, may not improperly be called an Electrometer. I have herewith sent you a drawing thereof [TAB. XX.] with the machine * to which I have fixed it.

* This portable machine is the contrivance of Mr. Read, mathematical instrument maker at Knightsbridge, near London.

FIGURE I.

A. The cylindrical glaſs of the machine, uſed inſtead of a globe. The cylindrical part of the glaſs is ſix inches in length, and ſixteen in circumference.

B. The wheel, at every turn of which the cylindrical glaſs revolves four times.

C. The conductor.

D. The coated phial.

E. A braſs wire loop, paſſing through the wood work to a tin plate, on which the coated phial ſtands.

F. The pillar of the Electrometer made of wood, bored cylindrically about $\frac{2}{3}$ of its length, and rendered electrical, by being long baked in an oven, and then boiled in linſeed oil, and again baked. At firſt the pillar was made of braſs, which, though it ſerved very well to determine the electric ſtroke for medical purpoſes, yet was defective in many experiments, as the table thereby became a ready conductor.

G. Braſs work, having its lower part incloſed within the bore of the pillar.

H. A ſcrew, which paſſes through the braſs work near the bottom, and fixes it in the pillar.

I. A groove for the ſcrew H to move in, when the Electrometer is moved higher or lower, as the different heights of different condenſing phials may require.

K. A well poliſhed hemiſpherical piece of braſs, fixed to the conductor.

L. A

L. A ſteel ſcrew, paſſing through the top of the braſs work, whoſe threads are diſtant nearly $\frac{1}{24}$ of an inch from each other.

M. A well poliſhed ſpherical piece of braſs, fixed to the ſcrew L, and oppoſite to K. The poliſh of K and M will often be deſtroyed by large electrical exploſions, and it ſhould again be reſtored; particularly where the experiments require accuracy.

N. A ſcale, with diviſions equal to each turn of the ſcrew.

O. A circular plate fixed to, and moving with the ſcrew, pointing at each turn to the diviſion upon the ſcale. This plate is alſo divided into twelve, to denote the parts of each turn.

The principle on which the Electrometer acts is very ſimple, being merely this; the coated phial is hereby rendered incapable of accumulating and retaining any more than a certain quantity of the electric fluid, for any intended experiment, when a metallic or non-electric communication is made from the ſcrew H to the wire loop E of the machine, and that quantity will be proportionate to the diſtance of K and M from each other, and conſequently the exploſion and ſtroke will thereby be regulated.

Thus if a perſon holds a wire faſtened to the ſcrew H in one hand, and another wire fixed to the loop E in the other, he will perceive no ſtroke, if K and M are in contact, notwithſtanding the cylindrical glaſs A acts ſtrongly. But if, by turning the ſcrew L, the ball M is diſtant from K $\frac{1}{100}$ part of an

an inch, a very ſmall ſtroke will be perceived, with an exploſion from K to M; and if K and M are diſtant one inch from each other, the quantity of the electric fluid, at the time of the exploſion, will be increaſed 100 times: for example, it appears by experiment, that, if the exploſion happens after 4 turns of the wheel B, when M is diſtant from K $\frac{1}{24}$ of an inch, or 1 turn of the ſcrew; the ſame will happen at 8 turns of the wheel, when M and K are diſtant 2 turns of the ſcrew, or $\frac{1}{12}$ of an inch; and if K and M are diſtant 3 turns of the ſcrew, the turns of the wheel will be 12 at the time of the exploſion; the ſame proportion will continue ſo far as the diſtance of K and M is equal to the condenſing power of the coated phial without waſting. By waſting, I mean when the phial is ſo fully charged, that part of the electric fluid eſcapes from the mouth of the bottle, or from the conductor into the air, or to ſome adjacent non-electric. The number of turns of the wheel, when K and M are at any of the above diſtances, will be more or leſs in proportion to the ſtate of the air, the cylindrical glaſs, the cuſhion againſt which the glaſs is rubbed, or the coated phial; which laſt will not give ſo great an exploſion when the air is damp as when dry.

The fewer the number of turns of the wheel, at any given diſtance, the better the machine worketh. Thus a comparative difference between any two machines may be determined.

A wire in general is better than a chain, unleſs the chain is held very tight; particularly in very ſmall ſtrokes, the electric fluid will be loſt in paſſing from link to link of the chain.

By

By experiment it alſo appears, that the quantity of electric fluid, at every exploſion, will be proportionate to the quantity of coated glaſs, either as to the ſize of the coated phial, or to the number of phials added. For example, if the phial D has half of the coating on each ſide of the glaſs taken off, the exploſion will happen after half the number of turns of the wheel, at any of the above diſtances; and if a phial, with twice the quantity of coated glaſs, is employed inſtead of D, the number of turns of the wheel will be double; the ſame will happen if two coated phials, each equal to D, are uſed; and if three phials, the number of turns will be triple, &c.

The phial D, uſed in the following experiments, contains about 80 ſquare inches of coating on the inſide, and alſo on the outſide of the glaſs; the mouth being ſtopped with wood, prepared like the pillar, and the coating not too near the mouth of the phial, to prevent the electric fluid's waſting, and thereby the phial may be more fully charged.

As K is part of the conductor, and of M the electrometer, the diſtance between them is the diſtance of the electrometer from the conductor; whence it will be readily underſtood, when I relate the diſtance of the electrometer, in any experiments. For example, the electrometer at 20, that is, M, is 20 turns of the ſcrew diſtant from K, or $\frac{20}{24}$ of an inch.

That lightning and electricity are of very near affinity, if not the ſame, evidently appears from the many diſcoveries you have made; and as the following experiments tend to confirm the ſame, as well as to

to illustrate the use of the electrometer, I hope they will not be unacceptable.

Experiment I.

A piece of moist tobacco-pipe clay, rolled cylindrically, *a.* fig. 2. about an inch in length, and about 2 or $\frac{3}{10}$ of an inch in diameter, having a piece of wire thrust into each end, *b b*, distant about $\frac{1}{10}$ of an inch from each other, with the solid clay between, and the end of one of the wires, *c c*, fixed to the loop of the machine E, and the other fixed to the small screw of the electrometer H, will, with an explosion at 20 of the electrometer, be inflated as in fig. 3. or if the clay is too dry, or the quantity of electricity too great, it will burst in pieces, leaving only the clay concave near the ends of the wires; and though the experiment will in appearance differ, yet it will always leave evident signs of an explosive power, or sudden rarefaction, excepting when the wires in the clay are at too great a distance from each other; then the electric fluid will only run over its moist surface. If, instead of clay, a mucilaginous vegetable paste is used, as wheat-flower and water, &c. the experiment will appear the same.

Experiment II.

Take a piece of common tobacco-pipe hard-baked, as used for smoaking, about an inch in length; fill the bore with clay, and put wires into each end, as in fig. 2. which applied in the same manner to the machine, will burst into many pieces, at

at 20 of the electrometer; ſometimes the pieces will be driven near ten feet from the machine.

Experiment III.

A ſmall ſquare piece of Portland ſtone, with holes drilled at each end ſo as to admit the wires, was in like manner burſt in pieces, when a ſecond coated phial was added to increaſe the ſtroke.

The iron cramps in ſtone buildings are ſimilar to the wires, and when a building is ſtruck by lightning produces a ſimilar effect. I obſerved, that when the tobacco pipe, or ſtone, was damp, the experiment ſucceeded better than when dry; and I frequently found, that either of them, after being firſt dipped in water, would be broken with a leſs exploſion than before.

This obſervation is different from the received opinion of many, not well acquainted with electricity, that lightning is leſs likely to do miſchief after a ſhower of rain than before: ſo far may be true, that the rain will bring down ſome of the lightning, and alſo render thatched houſes, &c. leſs likely to take fire, but will not aſſiſt buildings that have metallic ornaments near their tops, as the weather-cocks of churches, &c.

As a metallic conductor from the tops of buildings to the earth will prevent the effects of lightning on them, ſo will the ſmalleſt wire prevent the effects of electricity on the ſtone, or tobacco-pipe, when in contact with the two wires, *c c*, fig. 2.

If the tobacco-pipe, inſtead of clay, is filled as above, with an electric ſubſtance, as wax, powdered glaſs, or with any non-electric ſubſtance, inferior to metals as a conductor, it will be burſt in pieces with nearly the ſame quantity of the electric fluid.

As the above experiments ſucceeded better when the ſtone or clay were previouſly dipped in water than before, I was induced to try water only.

Experiment IV.

Having made a hole, without any cracks on the ſide, through the bottom of the phial, *a*, fig. 4. which may eaſily be done if the phial is conical at the bottom, as in the figure, by holding the phial inverted in one hand, and with the other ſtriking a pointed ſteel wire againſt the apex of the cone.

Through this hole I paſſed a wire, *b*, and filled the bottom, *c*, with melted ſealing wax, leaving the other end of the wire out, at *d*; when the wax was cold, the phial was about $\frac{3}{4}$ filled with water, and ſtopped with a cork, through which a wire, *e*, was paſſed downwards, till the points of the two wires were diſtant from each other about $\frac{1}{10}$ of an inch, as near as my eye could determine a wire from the electrometer was fixed to *e*, and another from the loop of the machine was fixed at *d*; by an exploſion, at 20 of the electrometer, the phial burſt

in

in pieces, the top falling from the bottom near the point of the lower wire. Another phial was fitted in the ſame manner, and the cork cut longitudinally, that the air might freely paſs at the time of the exploſion, but this made no ſenſible difference: often times the phial is ſo cracked as to reſemble radii from a center.

If oil is uſed inſtead of water, the event will be the ſame.

The quantity of electricity neceſſary to burſt the phial, appears to vary more in proportion to its thickneſs than its ſize; many phials of various ſizes may be broken at 10 of the electrometer, while others, nearly of the ſame ſize, remain ſound, with a ſtroke at 30, or even more.

I generally found green glaſs more difficult to break than white.

When the phial is not broken by the electric ſtroke, the agitation of the water may be ſenſibly obſerved at the inſtant of the exploſion, and the electric ſpark evidently ſeen to paſs through the water, from the point of one wire to the other.

This remarkable appearance of the electric fluid's paſſing through water may be obſerved, when the electrometer is at a ſmaller diſtance from the conductor, if the wires are nearer to each other.

I have broken many phials by the electric ſtrokes as above-mentioned, when the wires have been at the various diſtances, of above 1 inch to $\frac{1}{20}$ of an inch from each other, as near as my eye could determine; but the diſtance of about $\frac{1}{10}$ of an inch I uſually prefer.

 The

The above experiments I have often repeated, and may therefore be relied on: want of leisure has prevented me from pursuing them more minutely. But I hope they will serve as hints to others of more abilities and leisure, than

Your respectful

humble servant,

T. Lane.

Received

Received in October, 1767.

XLV. *Of the Increaſe and Mortality of the Inhabitants of the Iſland of* Madeira. *By Dr.* Thomas Heberden, *F. R. S.*

Read Nov. 26, 1767.

WHEN I conſider the number of people in the Madeira, and the ſtate of the inhabitants, I know no place more proper for forming an eſtimate of the increaſe and mortality of mankind, than this iſland; for the number of perſons is upwards of 60,000, all of whom may be ſuppoſed to live and die in the ſame place where they received their exiſtence; the acceſſion of ſtrangers and the egreſſion of the natives being ſo equally inconſiderable, that if the one doth not exactly counterbalance the other, the difference may juſtly be neglected, as of no conſequence in the general calculation.

This has excited my curioſity; and, by my intereſt with the vicar-general of this dioceſe, I have procured a ſurvey from houſe to houſe in each of the reſpective pariſhes; from which, and the pariſh regiſters, I have deduced the adjoined account.

An Hypotheſis.

The number of perſons in this iſland, in the year 1743, was 48234 of ſeven years old and upwards.

 Now

Now ſuppoſing the minors were in the ſame proportion then, as in this preſent year, the total of the inhabitants was 53,057. Therefore, by the rule of anatociſm, they have increaſed at the rate of 1.0082 *per cent. per annum*; and by the ſame rule do double in 84 years 4 months and 25 days.

From an exact ſurvey, made in the beginning of the year 1767, the number of inhabitants on the iſland of Madeira, was as follows;

Perſons of ſeven years old and upward	58669
Perſons under ſeven years of age	5945
Total	64614

Anno	Chriſtened Males	Chriſtened Fem.	Buried Males	Buried Fem.	Wedd.
1759	1021	905	542	594	438
1760	1198	1111	643	713	421
1761	1035	1022	837	909	513
1762	1128	1125	662	704	491
1763	1118	1115	540	578	476
1764	1112	1085	620	705	469
1765	1183	1143	618	649	495
1766	1172	1138	506	531	462
	8967	8644	4968	5383	3765
Total	17611		10351		

Chriſtened

Chriſtened in 8 years	17611	Medium for each year	$2201\frac{3}{8}$
Buried in 8 years	10351	Medium for each year	$1293\frac{7}{8}$
Octennial increaſe	7260	Annual increaſe	$907\frac{4}{8}$

Proportion of the yearly births to the number of perſons,	as	1	to	29,35
——— of the yearly burials to the number of perſons,	as	1	to	49,89.
——— of births to burials ———	———	100	to	58,77
——— of males born, to females ———	———	100	to	96,39
——— of females buried, to males ———	———	108,33	to	100

Weddings each year, at a medium	———			$470\frac{5}{8}$
Proportion of Weddings to births,	as	1	to	4,68
——— of Weddings to burials,	as	1	to	2,75

Mortality of the Seaſons.

Winter		Spring		Summer		Autumn	
January	93	April	108	July	129	October	87
February	84	May	105	Auguſt	135	November	111
March	132	June	120	September	84	December	84
	309		333		348		282

The mortality of ſpring and ſummer, to that of autumn and winter, as 115 to 100.

N. B. This calculation of the mortality of the ſeaſons is not deduced from the whole number of Inhabitants on the iſland, as I could not procure authentic materials to proceed with exactneſs; the number of the perſons, from which it is calculated, is 6880.

Received

Received November 19, 1767.

XLVI. *An Account of ſome very large Foſſil Teeth, found in* North America, *and deſcribed by* Peter Collinſon, *F. R. S.*

Read Nov. 26, 1767.

I Perſwade myſelf it will not be unacceptable to this Learned Society, to receive the beſt intelligences I can collect of the teeth, and bones of elephants, found in North America, in the year 1766, which are now offered for your inſpection.

George Croghan, Eſquire, who is a deputy of Sir William Johnſon, the King's ſuperintendant of Indian affairs in America, in the courſe of his navigation down the great river Ohio, after paſſing the Miame river, in the evening came near the place where the elephants bones are found, about four miles ſouth-eaſt of the Ohio, and about ſix hundred miles diſtant from and below Pitſburgh, from the neareſt ſea-coaſt at leaſt ſeven hundred miles. Next morning he met with a large road, which the buffaloes had beaten, wide enough for two waggons to go a-breaſt, leading ſtrait into the great licking-place, to which the buffaloes and all the ſpecies of deer reſort, at a certain ſeaſon of the year, to lick the earth and water from ſalt ſprings, that are impregnated with nitreous particles; whether to cleanſe their

their ſtomachs, or for what other purpoſe, is ſubmitted to the ſentiments of the Society.

Eſquire Croghan had been here ſome years before, and gave ſome account of the monſtrous bones, and teeth, found at this place, called by the Indians The Great Buffaloes Lick; but being now more at leiſure, he carefully examined all its ſurrounds, and diſcovered under a great bank, on the ſkirts of the Lick, five or ſix feet below the ſurface, open to view, a prodigious number of bones and teeth, ſpecimens of which now lie before the Society, belonging to ſome of the largeſt-ſized animals; by the quantity, he computes there could not be leſs than thirty of their ſkeletons.

By their great teeth, or tuſks, of fine ivory, ſome near ſeven feet long; every one that views them, I believe, will not heſitate to conclude they belong to elephants.

It is very remarkable, and worthy obſervation, none of the molares, or grinding teeth of elephants, are diſcovered with theſe tuſks; but great numbers of very large pronged teeth of ſome vaſt animals are only found with them, which have no reſemblance to the molares, or grinding teeth, of any great animal yet known.

As no living elephants have ever been ſeen or heard of in all America, ſince the Europeans have known that country, nor any creature like them; and there being no probability of their having been brought from Africa, or Aſia; and as it is impoſſible that elephants could inhabit the country where theſe bones and teeth are now found, by reaſon of the ſeverity of

 the

the winters, it ſeems incomprehenſible how they came there.

I conclude, many of this learned Society are not unacquainted with the foſſil elephants teeth annually found in Siberia, lodged in the banks of the great river Oby, and other rivers of that country.

On the ſyſtem of the deluge, it has been conjectured, that, as the extenſive kingdom of Siberia lies behind the native country of the elephants in Aſia, from Weſt to Eaſt, and to the North, by the violent action of the winds and waves, at the time of the deluge, theſe great floating bodies, the carcaſes of drowned elephants, were driven to the Northward, and, at the ſubſiding of the waters, depoſited where they are now found. But what ſyſtem, or hypotheſis, can, with any degree of probability, account for theſe remains of elephants being found in America, where thoſe creatures are not known ever to have exiſted, is ſubmitted to this learned Society.

Nov. 4,
1767.

P. S. The Biſhop of Carliſle preſented to the Royal Society, on the 27th of February, 1766, ſome foſſil teeth and bones from Peru, which have ſome analogy with the before-mentioned, not ſo recent, but much more petrified; the pronged teeth are like to agate.

A List of the Teeth and Bones sent over by George Croghan, *Esquire*, February 7, 1767, *from* Philadelphia.

To Lord Shelburne.

Two of the largest tusks, or teeth, one whole and entire, above six feet long, the thickness of common elephants teeth of that length.

Several very large forked or pronged teeth; a jaw-bone, with two of them in it.

To Doctor Franklin.

Four great tusks, of different sizes.

One broken in halves, near six feet long.

One much decayed, the center looks like chalk, or lime.

A part was cut off from one of these teeth, that has all the appearance of fine white ivory.

A joint of the vertebræ.

Three of the large pronged teeth; one has four rows of fangs.

Besides the above, Captain Owry, an Officer who served in the country during the last war, now living at Hammersmith, hath a small tusk, as if of a calf elephant, the surface of a fine shining chesnut colour, and a recent look; and a great pronged tooth, larger than any of the above, which were also brought from the same licking place.

XLVII. *Sequel to the foregoing Account of the large Fossil Teeth. By* P. Collinson, *F. R. S.*

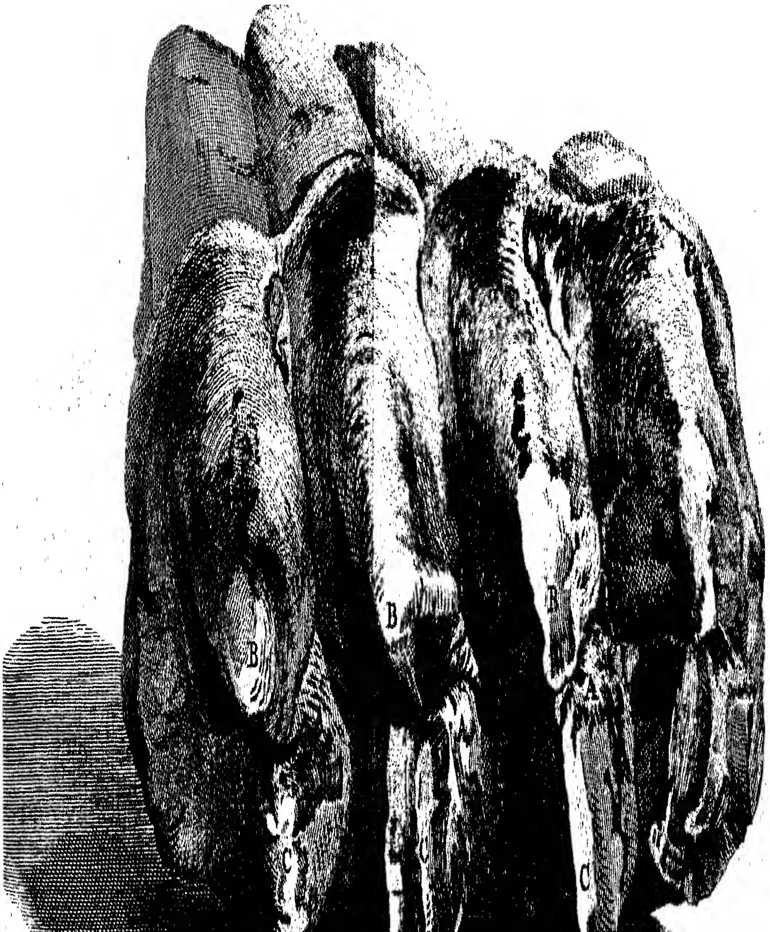
Read Dec. 10, 1767.

IN my obſervations on the long teeth and grinders, at the laſt meeting of this Society, I forebore giving my ſentiments on theſe remains of great animals found at the Great Lick, near the river Ohio, being willing the Society ſhould determine for themſelves.

As I perceived one of the long teeth, or tuſks, was channelled or ribbed, near the larger end, I was in ſome doubt, if peculiar to the elephant. To ſatisfy myſelf, I went to a warehouſe, where there were teeth of all ſorts and ſizes for ſale; on examining them, I found as many ribbed or channelled, as plain and ſmooth, ſo that now, I have no difficuty to pronounce them, agreeing in all reſpects, with the elephants teeth from Africa and Aſia.

But as the biting or grinding teeth, found with the others, have no affinity with the molares of the elephant, I muſt conclude, that they, with the long teeth, belong to another ſpecies of elephant, not yet known; or elſe that they are the remains of ſome vaſt animal, that hath the long teeth, or tuſks, of the elephant, with large grinders peculiar to that ſpecies, being different in ſize and ſhape from any other animal yet known. I had one of theſe grinders, that weighed near four pounds, with as fine an enamel on it, as if juſt taken out of the head of the creature.

The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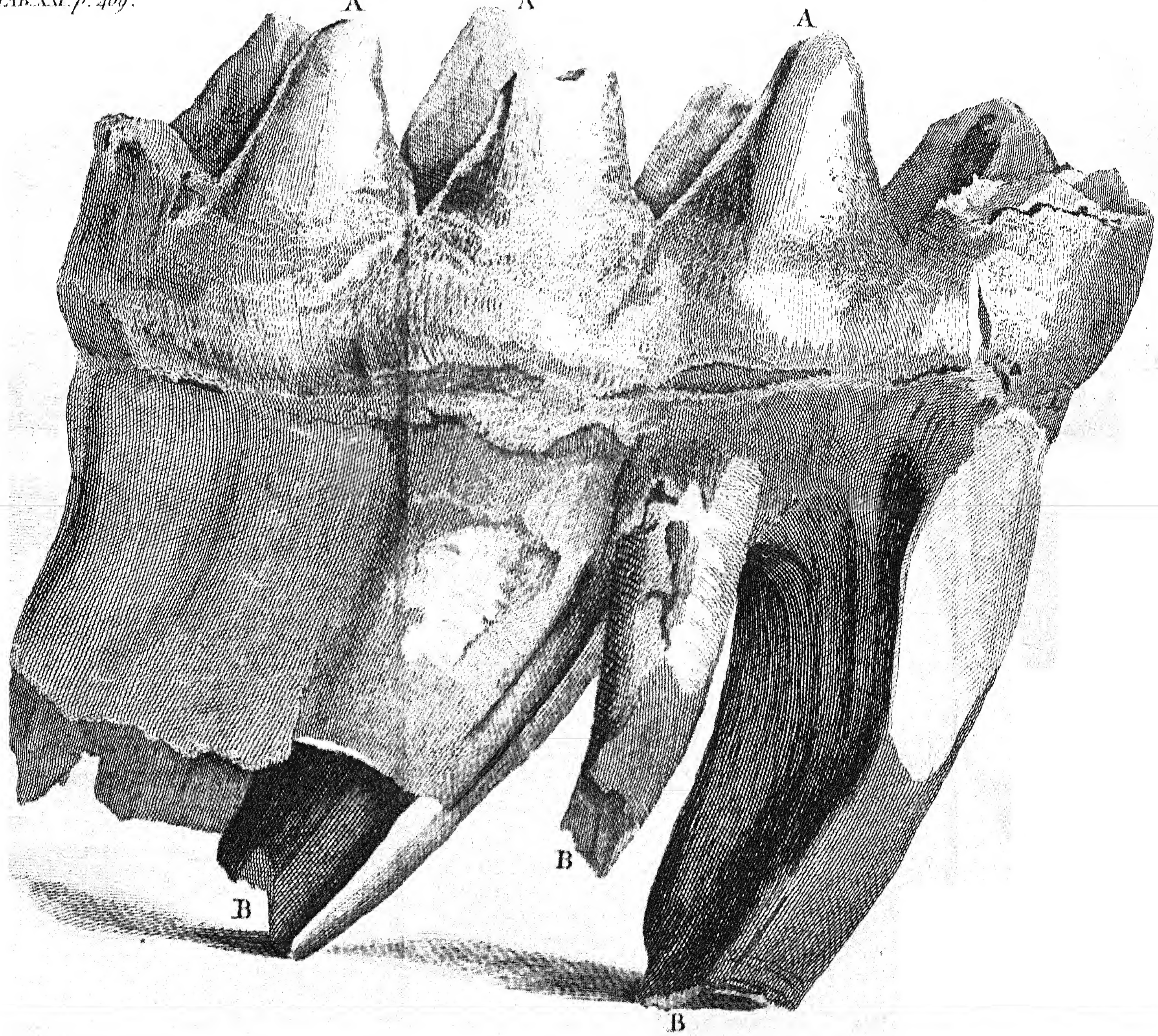
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The elephant is wholly ſupported by vegetables; and the animal to which theſe grinding teeth belong, by their make and form, ſeemed deſigned for the biting and breaking off the branches of trees and ſhrubs for its ſuſtenance; and if I may be allowed to conclude from analogy, that the great heavy unweildy animals, ſuch as elephants, and the rhinoceros, &c. are not carnivorous, being unable, from want of agility and ſwiftneſs, to purſue their prey, ſo are wholly confined to vegetable food; and for the ſame reaſon, this great creature, to which theſe teeth belong, wherever it exiſts, is probably ſupported by browſing on trees and ſhrubs, and other vegetable food.

Explanation of the Fig. in Tab. XXI. and XXII.

Tab. XXI. is a ſide view of a large pronged tooth, that weighed three pounds and three quarters, and was eighteen inches round, and four inches thick. It is now in the poſſeſſion of the Right Honourable Earl Bute. A. A. A. The prominences of the top or crown. B. B. B. The prongs by which it has been faſtened in the ſockets.

Tab. XXII. repreſents the top of the tooth; A. A. A. are the ſinus's, or concavities between B. B. B. and C. C. C. or the two rows of prominences that form the crown of the tooth.

XLIX. *A Catalogue of the* Fifty Plants *from* Chelsea Garden, *presented to the* Royal Society *by the worshipful Company of Apothecaries, for the Year* 1766, *pursuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. et Soc. Reg. nuper Præses: By* William Hudson, *Societatis Regiæ & clariss. Societatis Pharmaceut.* Lond. *Soc. Hort.* Chelsean. *Præfectus et Prælector Botanic.*

Read Dec. 10, 1767.

2201 AGrostis *spica venti*, petalo exteriore exserente aristam rectam strictam longissimam. Lin. Sp. pl. ed. II. 91. Hudſ. Fl. Angl. 26.

Gramen segetum altissimum, panicula sparsa. Bauh. pin. 3.

2202 Agrostis *mil ea*, petalo exteriore arista terminali recta stricta mediocri. Lin. Sp. pl. ed. II. 91.

2203 Aira *cæspitosa*, foliis planis, panicula patente, petalis basi villosis aristatisque, arista recta brevi. Lin. Sp. pl. ed. II. 96. Hudſ. Fl. Angl. 29.

Gramen segetum, panicula arundinacea Bauh. pin. 3. Theatr. 35.

2204 Aira

2204 Aira *caryophyllea*, foliis ſetaceis, panicula divaricata, floribus ariſtatis diſtantibus. Lin. Sp. pl. ed. II. 97. Hudſ. Fl. Angl. 31.

Gramen paniculatum, locuſtis purpuro-argenteis, annuum. R. Syn. 407.

2205 Alopecurus *pratenſis*, panicula cylindrica ſpiciformi, glumis villoſis, culmo erecto. Hudſ. Fl. Angl. 23.

Alopecurus culmo ſpicato erecto, glumis villoſis. Lin. Sp. pl. ed. II. 88.

Gramen phalaroides ſpica molli. Bauh. pin. 4.

2206 Alopecurus *agreſtis*, ſpica cylindrica longiſſima, glumis glabris, culmo erecto. Hudſ. Fl. Angl. 23.

Alopecurus culmo ſpicato erecto, glumis nudis. Lin. Sp. pl. ed. II. 89.

Gramen typhoides, ſpica anguſtiore. Bauh. pin. 4.

2207 Alopecurus *geniculatus*, culmo ſpicato infracto. Lin. Sp. pl. ed. II. 89. Hudſ. Fl. Angl. 24.

Gramen aquaticum geniculatum ſpicatum. Bauh. pin. 3.

2208 Alopecurus *bulboſus*, ſpica cylindrica, culmo erecto, radice bulboſa. Hudſ. Fl. Angl. 24.

Gramen myoſuroides nodoſum. R. Syn. 397.

2209 Alopecurus *Monſpelienſis*, panicula ſubſpicata, glumis ſcabris, corollis ariſtatis. Lin. Sp. pl. ed. II. 89.

Gramen alopecuroides Anglo-britannicum maximum. Bauh. pin. 4.

2210 Alopecurus *paniceus*, panicula ſubſpicata, glumis villoſis, corollis ariſtatis. Lin. Sp. pl. ed. II. 90.

Gramen alopecurum minus, ſpica vireſcente divulſa. Barr. ic. 115. f. 1.

2211 Anthoxanthum *odoratum*, ſpica ovato-oblonga, floſculis ſubpedunculatis ariſta longioribus. Lin. Sp. pl. ed. II. 40. Hudſ. Fl. Angl. 10.

Gramen pratenſe ſpica flaveſcente. Bauh. pin. 3.

2212 Avena *ſativa*, paniculata, calycibus diſpermis, ſeminibus lævibus. Lin. Sp. pl. ed. II. 118.

Avena alba. Bauh. pin. 23.

2213 Avena *nuda*, paniculata, calycibus trifloris, receptaculo calycem excedente, petalis dorſo ariſtatis. Lin. Sp. pl. 118. Hudſ. Fl. Angl. 41.

Avena nuda. Bauh. pin. 23. R. Syn. 289.

2214 Avena *fatua*, paniculata, calycibus trifloris, floſculis omnibus baſi piloſis, ariſtis totis lævibus. Lin. Sp. pl. ed. II. 118. Hudſ. Fl. Angl. 41.

Avena ſylveſtris piloſa, ariſtis recurvis. Hiſt. Ox. III. 209. t. 7. f. 5.

2215 Avena *fragilis*, ſpicata, floſculis ſubquaternis calyce longioribus. Lin. Sp. pl. ed. II. 119.

Gramen loliaceum ſpurium hirſutum, ariſtis geniculatis. Barr. ic. 905. f. 1, 2, 3.

2216 Briza *eragroſtis*, ſpiculis lanceolatis, floſculis viginti. Lin. Sp. pl. ed. II. 103.

Gramen paniculis elegantiſſimis. Bauh. pin. 2. Hiſt. Ox. III. 204. t. 6. f. 52.

2217 Briza *pinnata*, ſpica diſticha, ſpiculis ovato-lanceolatis.

Gramen filiceum, paniculis integris. Bocc. Sicil. 62. t. 33. f. 2.

2218 Bromus *secalinus*, panicula patente, spiculis ovatis, aristis rectis. Lin. Sp. pl. ed. II. Hudf. Fl. Angl. 39.

Gramen avenaceum segetate majus, gluma turgidiore. Hist. Ox. III. t. 7. f. 17.

2219 Bromus *mollis*, panicula erectiuscula, spicis ovatis, aristis rectis, foliis mollissime villosis. Lin. Sp. pl. ed. II. 112.

Gramen avenaceum pratense, panicula squamata et villosa. Hist. Ox. III. 213. t. 7. f. 18.

2220 Bromus *rubens*, panicula fasciculata, spiculis subsessilibus villosis, aristis erectis. Lin. Sp. pl. ed. II. 114.

Gramen panicula molli rubente. Bauh. hist. II. 266.

2221 Bromus *giganteus*, panicula nutante, spiculis quadrifloris aristis brevioribus. Lin. Sp. pl. ed. II. 114. Hudf. Fl. Angl. 40.

Gramen sylvaticum glabrum panicula recurva. Vaill. paris. 93. t. 18. f. 3.

2222 Bromus *distachyos*, spicis duabus erectis alternis. Lin. Sp. pl. ed. II. 115.

Gramen spica brizæ minus. Bauh. pin. 9. Pluk. Phyt. t. 33. f. 1.

2223 Cynosurus *cristatus*, bracteis pinnatifidis. Lin. Sp. pl. ed. II. 105. Hudf. Fl. Angl. 47.

Gramen cristatum. Bauh. hist. II. 468. R. Syn. 398.

2224 Cynofurus *echinatus*, bracteis pinnato-paleaceis ariftatis. Roy. Lugdb. 64. Lin. Sp. pl. ed. II. 105. Hudf. Fl. Angl. 47.

Gramen alopecuroides fpica afpera. Bauh. pin. 4.

2225 Cynofurus *aureus*, paniculæ fpiculis fterilibus pendulis ternatis, floribus ariftatis. Lin. Sp. pl. ed. II. 107.

Gramen panicula pendula aurea. Bauh. pin. 3. theatr. 33. Scheuch Agr. 149.

2226 Feftuca *elatior*, panicula fecunda erecta, fpiculis linearibus muticis, floris planis. Hudf. Fl. Angl. 37.

Gramen loliaceum, fpica divifa, pratenfe majus. Hift. Ox. III. 184. t. 2. f. 15.

2227 Feftuca *pratenfis*, panicula fecunda erecta, fpiculis linearibus muticis, floris planis. Hudf. Fl. Angl. 37.

Gramen paniculatum elatius, fpicis longis muticis et fquamofis. R. S. 411.

2228 Feftuca *fluitans*, panicula ramofa erecta, fpiculis fubfeffilibus teretibus muticis. Lin. Sp. pl. ed. II. 111. Hudf. Fl. Angl. 38.

Gramen aquaticum cum longiffima panicula. Bauh. hift. II. 490. R. S. 412.

2229 Feftuca *bromoides*, panicula fecunda fpiculis erectis lævibus calycis altera valvula integra, altera ariftata. Lin. Sp. pl. ed. II. 111. Hudf. Fl. Angl.

Gramen paniculatum bromoides minus, panicula ariftatis unam partem fpectantibus. R. Syn. 415.

2230 Feftuca

2230 Festuca *ovina*, panicula secunda coarctata aristata, culmo tetragono nudiusculo, foliis setaceis. Lin. Sp. pl. ed. II. 108. Hudſ. Fl. Angl. 36.

Gramen capillaceum, locustis pennatis non aristatis. Pluk. ph. t. 34. f. 2. R. Syn. 410.

2231 Holcus *spicatus*, glumis bifloris muticis floribus geminis penicillo involucratis, spica ovato-oblonga. Lin. Sp. pl. ed. II. 1483.

Gramen paniceum sylvestre maximum, Indiæ orientalis. Pluk. Phyt. t. 32. f. 4.

2232 Holcus *Sorghum*, glumis villosis, seminibus aristatis. Lin Sp. pl. ed. II. 1484.

Milium arundinaceum, subrotundo semine, sorgho nominatum. Bauh. pin. 26. Hist. Ox. III. 196. t. 5. f. 7.

2233 Hordium *murinum*, flosculis lateralibus masculis aristatis, involucris intermediis ciliatis. Lin. Sp. pl. ed. II. 125.

Gramen secalinum chalepense, radice tuberosa. Hist. Ox. III. 179. t. 6. f. 7.

2235 Lolium *temulentum*, spica aristata, spiculis compressis aristatis. Lin. Sp. pl. ed. II. 122.

Lolium spica aristata, radice annua. Lin. Sp. pl. 83. Hudſ. Fl. Angl. 44.

Gramen loliaceum, spica longiore. Bauh. pin. 9. Theatr. 121.

2236 Lolium *perenne*, spica mutica, spiculis compressis multifloris. Lin. Sp. pl. ed. II. 122.

Lolium ſpicis muticis, radice perenne. Lin. Sp. pl. 83. Hudſ. Fl. Angl. 44.

Gramen loliaceum anguſtiore folio et ſpica. Bauh. pin. 9. Theatr. 121.

2237 Melica *ciliata*, floſculis inferioris petalo exteriore ciliato. Lin. Sp. pl. ed. II. 97.

Gramen avenaceum montanum lanuginoſum. Bauh. pin 10. pr. 20.

2238 Melica *nutans*, petalis imberbibus, panicula nutante ſimplici. Lin. Sp. pl. ed. II. 98. Hudſ. Fl. Angl. 31.

Gramen avenaceum locuſtis rarioribus. Bauh. pin. 10.

2239 Panicum *patens*, panicula oblonga patente, calycibus bifloris, foliis lineari-lanceolatis. Lin. Sp. pl. ed. II. 86.

Tſiama-pullu. Hort. Mal. XII. 75. t. 41.

2240 Panicum *miliaceum*, panicula laxa flaccida, foliorum vaginis pubeſcentibus. Lin. Sp. pl. ed. II. 86.

Milium ſemine albo et luteo. Bauh. pin. 8. Theatr. 139.

2241 Panicum *verticillatum*, ſpica verticillata, racemulis quaternis invocellis unifloris biſetis, culmo diffuſo. Lin. Sp. pl. ed II. 82.

Gramen paniceum ſpica ſimplici. Bauh. pin. 8. Theatr. 139.

2942 Panicum *viride*, ſpica tereti, involucellis bifloris faſciculato-piloſis, ſeminibus nervoſis. Lin. Sp. pl. ed. II. 83. Hudſ. Fl. Angl. 21.

Gramen paniceum, ſpica ſimplici. Bauh. pin. 8. Sch. Agr. 46.

2243 Panicum *colonum*, ſpicis alternis ſecundis muticis ovatis ſcabris, rachi teretiuſcula. Lin. Sp. pl. ed. II. 84.

Gramen paniceum minus ſpica divulſa inſulæ Barbadenſis. Pluk. Ph. t. 189. f. 5.

2244 Phalaris *canarienſis*, panicula ſubovato ſpiciformi: glumis carinatis. Lin. Sp. pl. ed. II. 79.

Phalaris major, ſemine albo. Bauh. pin. 28.

2245 Phalaris *utriculata*, panicula ſpicata, petalis ariſta articulata, vagina ſupremi folii ſpathiformi. Lin. Sp. pl. ed. II. 80.

Gramen pratenſe, ſpica purpurea ex utriculo prodeunte. Bauh. pin. 3. Theatr. 44.

2246 Phalaris *arundinacea*, panicula oblonga ventricoſa. Lin. Sp. pl. ed. II. 80. Hudſ. Fl. Angl. 21.

Gramen arundinaceum ſpicatum. Bauh. pin. 6. Theatr. 94.

2247 Poa *ciliaris*, panicula glomerata, glumarum valvulis interioribus piloſo-ciliatis. Lin. Sp. pl. ed. II. 102.

Gramen pratenſe, ſpicis brevibus muticis locuſtis minimis. Sloan. Hiſt. Jam. I. 114. t. 73. f. 1.

2248 Poa *Eragroſtis*, panicula patente, pedicellis flexuoſis, ſpiculis ſerratis decemfloris. Lin. Sp. pl. ed. II. 100.

Gramen phalaroides, ſparſa brizæ panicula, min. Barr. Ic. 44. f. 2.

2249 Poa *rigida*, panicula lanceolata ſubramoſa, floribus alternis ſecundis. Lin. Sp. pl. ed. II. 102. Hudſ. Fl. Angl. 35.

Gramen exile duriuſculum in muris et aridis proveniens. R. Syn. 410.

2250 Poa *compreſſa*, panicula ſecunda coarctata, culmo obliquo compreſſo. Lin. Sp. pl. ed. II. 101. Hudſ. Fl. Angl. 33.

Gramen paniculatum, radice repente, culmo compreſſo. Vaill. pariſ. 91. t. 18. f. 5.

Received November 12, 1767.

XLIX. *An Account of ſome neutral Salts made with vegetable Acids, and with the Salt of Amber; which ſhews that vegetable Acids differ from one another; and that the Salt of Amber is an Acid of a particular kind, and not the ſame with that of Sea Salt, or of Vitriol, as alledged by many chemical authors. By* Donald Monro, *M. D. Phyſician to His Majeſty's Army, and to* Saint George's Hoſpital. *F. R. S.*

Title read December 17, 1767.

Read January 14, 21, and 28, 1768.

THOUGH no ſubſtances have been more generally uſed, both for the preſervation of health, and the cure of diſeaſes, than vegetable acids, yet hitherto they have been examined with ſo little care, that it has been the common received opinion, that they were all nearly of the ſame nature, at leaſt as to their chemical properties, and poſſeſſed nearly of the ſame virtues; but the following account of neutral ſalts, made with theſe acids, and the foſſil or mineral alkali, ſhews that they differ materially from one another.

 But

Previous, however, to entering into the account of theſe ſalts, it will be proper to mention ſome few things relative to ſalts in general.

Simple ſalts are commonly divided into *acid* and *alkaline*.

The *acid* are reckoned four in number.

1. The vitriolic.
2. The nitrous.
3. The marine, or muriatic.
4. And the vegetable.

The *alkaline* three.

1. The vegetable, or that which is got from the aſhes of moſt vegetable ſubſtances.

2. The foſſil, or mineral, called likewiſe ſoda and natrum, which is got either by burning certain marine plants; or from ſea ſalt; or in the bowels of the earth.

3. The volatile, which is got either by putrefaction, or by the force of fire, from moſt animal ſubſtances; or by diſtillation from muſtard ſeed, and ſome other particular vegetables.

The *acid* are diſtinguiſhed from each other, by their taſte, ſmell, and other properties, but principally by their forming different neutral ſalts with the ſame alkali.

And the *alkaline* are known likewiſe from one another, by their forming different neutral ſalts, when joined with the ſame acid.

Hence when we find *acid*, or *alkaline* ſalts, in different bodies, if we ſaturate each with the ſame alkali, or with

with the ſame acid, according as the original ſalt is of an acid, or of an alkaline nature, and find upon diſſolving, evaporating, and cryſtalliſing the neutral ſalts, that they are all of the ſame kind, we conclude, that the original acid, or alkali, was the ſame in all; but if we obtain different neutral ſalts from each, we conclude that the original acid, or alkali, was different in each.

If there are no more alkaline ſalts in nature, than the three already mentioned; and if there were no more acids than four; then the number of neutral ſalts would be confined to the twelve marked in Dr. Cullen's Table; but it will appear from the following experiments, that inſtead of one, there are many vegetable acids; and that, therefore, the number of true neutral ſalts muſt be greatly multiplied *.

* Dr. Cullen's Table of neutral Salts.

Acid	Alkaline	Neutral Salt	Acid	Alkaline	Neutral Salt
Vitriolic	Vegetable	Vitriolic Tartar	Marine or Muriatic	Vegetable	Sal. digeſt. Sylvii
	Foſſil	Glauber Salt		Foſſil	Common Salt
	Volatile	Vitriol. Ammoniac		Volatile	Common Ammoniac
Nitrous	Vegetable	Common Nitre	Vegetable	Vegetable	Regenerated Tartar
	Foſſil	Cubic Nitre		Foſſil	Rochelle Salt
	Volatile	Nitrous Ammoniac		Volatile	Spiritus Mindereri

Dr. Vogel, Profeſſor of chemiſtry in the univerſity of Gottengen, in his Inſtitutiones Chemiæ, publiſhed in 1752, gives a table of neutral ſalts, which comprehends the twelve mentioned by Dr. Cullen, with the addition of three or four more. He ſeems to believe, that the acid of vitriol forms a different neutral ſalt with the pot-aſh, and with the alkali procured from nitre. He mentions three ſalts, made with the vegetable alkali

 Many

Many chemists have affirmed, that the vitriolic is the only original acid in nature; and that the nitrous and marine are only this acid changed into different forms by foreign mixtures; and Dr. Boerhaave *, Vogel †, Macquer ‡, and most late chemists, seem to think, that as all trees, plants, and other vegetable substances, receive their nourishment from the bowels of the earth, therefore their acids are only some of the mineral changed into a different form by the vegetative process; and that they all approached in their nature either to the vitriolic, the nitrous, or the marine: and as the neutral salts, produced from the mixture of the vegetable alkali, with vinegar, cream of tartar, and other common vegetable acids, have a good deal of the same external appearance, most chemists have concluded, that all vegetable acids were nearly of the same nature; though some few have suspected, that they might be found to differ from one another, and to have different degrees of affinity, if they were examined with care; and to confirm this, Dr. Vogel § tells us, that if some of the Rochelle salts be thrown into a decoction of tamarinds, the alkaline basis of the Rochelle salt will unite with the acid of the tamarinds, and the cream of tartar will be precipitated.

and vegetable acids; to wit, with vinegar, crystals of tartar, and lemon; and one with the native acid salt (as he calls it) of urine, and the volatile alkali, and one with the acid of tartar, and the volatile alkali.

* Boerhavii Element. Chemiæ, vol. I. 804.

† Vogel, Institut. Chemiæ, p. 215. sect. 468. Ed. II.

‡ Macquer, Elemens de Chymie Theorique, chap. xvi. p. 240.

§ Vogel, Institut. Chemiæ, p. 216. sect. 469.

As I always suspected, from the taste and smell that vegetable acids differed materially from one another, and was the more confirmed in this opinion by the above experiment mentioned by Dr. Vogel, I began to consider whether some method might not be fallen upon to determine this question; and, on recollecting, that the Rochelle salt * concreted into large solid crystals, which preserved their figure long, even in the open air, though the tartarus tartarisatus † always appeared in the form of a powdery foliated salt, and run very soon *per deliquium*, when exposed to the air, I imagined, that if we were to unite the fossil, or mineral alkali, with different vegetable acids, we should be able to obtain true neutral salts in form of regular crystals; which would shew how far these acids differed or approached to each other in their nature and properties; and, upon trial, found that I had judged right; for each particular acid almost yielded a neutral salt pecauliar to itself, of which I shall now give a particulr account, and shall range these salts under the following heads.

1. Of neutral salts formed with native vegetable acids.

2. Of neutral salts formed with fermented vegetable acids.

* The Rochelle salt is made with the crystals of tartar, and the fossil alkali.

† The tartarus tartarisatus, with the crystals of tartar, and the salt of tartar; so that the only difference between these two salts is, that the one is made with the fossil, and the other with the vegetable alkali.

3. Of neutral ſalts formed with diſtilled vegetable acids.

4. Of neutral ſalts formed with flowers of benzoin and ſalt of amber.

S E C T. I.

Of neutral ſalts formed with native vegetable acids, and the foſſil or mineral alkali.

Experiment I.

With the acid of lemons.

The firſt experiment I made was with the acid of lemons; ſix ounces of the juice ſaturated rather more than three drams of the foſſil alkali; and upon evaporating the liquor to a pellicle, and letting it ſtand for ſome days, I obtained a ſalt compoſed of a number of ſmall cryſtals of irregular figures; ſome appeared to be irregular ſquares, or rhomboidal; others irregular pentagons; others to have more ſides; but this general appearance was nearly what is repreſented at *a. a. a.* &c. in Tab. XXIII. fig. 1. They were moſtly flat, and not above $\frac{1}{8}$ or $\frac{1}{10}$ of an inch thick; though ſome few were ſomewhat of an oblong irregular cubical ſhape, if I may be allowed to uſe the expreſſion.

Having obſerved that the figure of neutral ſalts, made with vegetable acids, varied ſometimes, according as they were cryſtalliſed in larger or leſs quantity, I got a quart of lemon juice, and ſaturated it with about two ounces, and two drams of the foſſil

foſſil alkali. Before adding the alkaline ſalt in this experiment, I tried the temperature of the lemon juice with one of Fahrenheit's thermometers, and found that the quickſilver ſtood in the tube at 54; upon removing the thermometer, I immediately added the alkaline ſalt; and as the ſolution was begun, I again put the thermometer into the liquor, and let it remain for above a minute, and the quickſilver ſunk above one degree; ſo that this acid generates cold in the time of its uniting with the foſſil alkali, though the neutral ſalt, produced from their union, does not affect the thermometer in the time of its ſolution in water.

The appearance of the ſalt obtained in this cryſtalliſation was very different from what it was in the former. The whole was made up of an infinite number of chryſtals, ſo ſmall that one at firſt could ſcarce diſtinguiſh their figure; but on examination part ſeemed to be of the ſame ſhape as the larger ones, got in the former proceſs; the others were very ſmall oblong parallelograms, and they were every where interſperſed with a number of ſmall longiſh cryſtals, which in many places lay acroſs each other, and formed a kind of lattice work. The general appearance of this cryſtalliſation is repreſented by *b. b.* &c. in Tab. XXIII. fig. 1. and that of ſome of the particular cryſtals by *c. c. c.*

The taſte of this ſalt is very mild, and rather pleaſant, approaching a little to that of a very weak ſea ſalt.

Expe-

Experiment II.

With the acid of limes.

The lime is a fruit of the ſame genus as the lemon; its acid is ſharper, and has a more agreeable flavour. From the near reſemblance of theſe two fruits, one ſhould have ſuſpected that the neutral ſalt of both would have been almoſt the ſame; but their appearance is ſomewhat different, though perhaps upon more accurate trials they may be found to have nearly the ſame virtues, and chemical properties.

The firſt experiment I made was with the juice of a dozen and a half of ſmall limes; and the neutral ſalt, produced from thence, was of the ſame ſhape, figure, and appearance, as the larger cryſtals obtained in the firſt experiment with the lemons; only the cryſtals were much ſmaller, and ſuch as repreſented by *a. a.* &c. fig. 2. But having afterwards procured three dozen of larger and finer limes, I got from them near three times the quantity of juice I had in the former proceſs; and having ſaturated this with the alkali, evaporated and cryſtalliſed it, I obtained a ſalt very different in its appearance from the former; though, in other reſpects, it ſeemed to be intirely of the ſame nature. Its cryſtals were of the ſize, and ſomewhat of the appearance, of barley corns, or grains of wheat, as at *c. c. c.*; ſome larger, ſome ſmaller; and laid in an irregular manner, but ſo as to form a beautiful cryſtalliſation, which is repreſented by *b. b. b.* fig. 2. They appeared,

peared, at a little diſtance, to be roundiſh, but on examining narrowly, their ſides were found to be made up of five or ſix flat ſurfaces; and generally one end of each cryſtal was made up of two flattiſh ſides, which roſe like a wedge which did not come quite to a point, but left a ſmall narrow ſurface between.

Theſe chryſtals, in the mouth, impreſs at firſt a very ſlight ſaltiſh, and ſomewhat ſweetiſh cool taſte; which is by no means unpleaſant, and reſembles a good deal that of the ſalt of lemons. They did not affect the thermometer in the time of their ſolution in water.

Experiments III. and IV.

With the acid of Sevill oranges, and of peaches.

It being late in the ſummer before I made any experiment with the juice of the Sevill oranges, I could get none of this fruit but what had been long kept and was ſhrivelled, in ſo much that a dozen and a half of the oranges did not yield more than half a pint of juice, which had loſt a great deal of its acidity, and ſaturated but a very ſmall quantity of the alkali; and on cryſtalliſing I could obtain no other ſalt but a few very ſmall cubical or ſquare cryſtals, ſuch as are repreſented by fig. 3. and ſimilar to the ſalt got in an experiment I made with peaches, as may be ſeen in fig. 4. A ſaponaceous or mucous matter, with which theſe ſaturated juices abounded, ſeemed to prevent the cryſtalliſation of the ſalts.

As both the juice of the orange, and of the peaches, was in ſmall quantity, and not in the moſt proper ſtate for yielding a neutral ſalt, theſe experiments ought to be repeated, before we can ſay what is the natural figure and appearance of the ſalts, that may be got by ſaturating the juices of theſe fruits with an alkali.

Experiment V.

With the acid of currants.

A quart of the juice of white currants, after being ſaturated with about nine drams of the foſſil alkali, and purified by repeated filtrations, was evaporated till a pellicle appeared; being put into a cool place, and allowed to ſtand for two or three days, it yielded a number of ſmall ſquare flattiſh cryſtals, ſuch as are repreſented by fig. 5. Many of them ſeemed to be exact ſquares, and in general they approached nearer to this figure, than the cryſtals of any of the other neutral ſalts I have hitherto met with.

This ſalt approached in its taſte to that of the limes; its cryſtals were hard and firm, and did not run *per deliquium*.

Experiment VI.

With the acid of gooſeberries.

A quart of the juice of gooſeberries, being treated in the ſame way, as that of the currants, yielded a neutral

neutral ſalt very different in its appearance, from any of thoſe hitherto mentioned. Its baſis, or what adhered to the tea-cup, was made up of a number of very ſmall roundiſh or ſquariſh cryſtals; which formed an incruſtation thicker than a ſhilling; from which grew up a number of very fine, thin, tranſparent plates, of irregular ſhapes; they were narrower at the baſis than above; and in ſome meaſure might be compared to the ſcales of a ſmall fiſh, or the wings of flies, ſet on their edges at a little diſtance from one another; in ſome places the plates aroſe from the ſides of others; and in others they appeared ſomewhat like the fine leaves of very ſmall plants. In fig. 6. we have different views of this ſalt; *a. a.* repreſents a piece of cryſtalliſed ſalt viewed from above; *b. b.* a profile view of the thin plates ſtanding on their baſis; *c. c.* the baſis itſelf; and the letters *d.* a view of the thin plates laid on their flat ſides.

Experiment VII.

With the acid of apples.

Having got two dozen of codling apples, I cut them to pieces, put them into a large earthen veſſel, and poured three quarts of water upon them; and then diſſolved above two ounces of the foſſil alkaline ſalt in the water, and let them ſtand for ſix days; on examining, I found the water to be nearly in a neutral ſtate; it did not ferment on the addition either of an acid or of an alkali. I then filtered the liquor through paper, and evaporated it,

till

till it was reduced to about five or ſix ounces, when it became thick, and a pellicle began to form on the ſurface. I then ſet it in a cool place, to allow the ſalts to concrete. After two days were elapſed, it was covered with a blueiſh variegated ſaline cruſt; immediately below which was a clay coloured ſaline matter, which reſembled wet earth or ſand, that had been raiſed by ſmall worms; and this was interſperſed every where with ſmall flattiſh globules of the ſame ſort of matter; below this was a purpliſh jelly, interſperſed with a whitiſh or aſh coloured ſaline matter, formed into irregular longiſh flat plates, which looked more like a compoſition of ſalt and earth than a pure ſalt. The appearance of this ſaline matter made me ſuſpect, that it was mixed with ſome ſort of oil, which the alkaline ſalt had extracted from the ſkins of the apples, which I had forgot to peel off before infuſing them in water; I therefore got a freſh parcel of codling apples, which I cauſed to be carefully peeled, and then treated them in the ſame manner as the former, and obtained the beautiful ſalt painted in fig. 7. which reſembled a good deal the ſalt of the gooſeberries; being compoſed of a number of ſmall roundiſh very delicate tranſparent plates, ſtanding on one edge, on a fine ſaline cruſt, which adhered every where to the ſides of the china baſon; and were inteſperſed with a grey coloured ſaline matter. The cryſtals of this ſalt were in general rather ſmaller, rounder, and more of a ſize, than thoſe of the gooſeberry; and I did not obſerve any riſing from the ſides of others as in it; and they ſeemed to be diſpoſed in a more regular uniform manner.

The

The letters *a. a.* &c. reprefent pieces of this cryftallifation viewed from above; *b. b.* &c. fome of the fine plates laid on their fide; *c. c.* and *e* fome of the afh coloured plates obtained in the firft operation; *d. d.* fome of the brown clay coloured faline matter; *f.* the flattifh globules, which befet every where the infide of the pellicle, that was on the top.

After the cryftallifation of the falt in this fecond procefs, the liquor which remained was poured into another fmall china bafon; and, on being evaporated, exhibited nearly the fame appearances as had been obferved in the firft procefs.

EXPERIMENT VIII.

With the acid of wild forrel.

In order to fave the trouble of a tedious evaporation, by faturating this acid mixed with the other juices of the plant, I procured fome of the effential falt of the wild forrel, from Mr. Heineken, apothecary in Duke-ftreet; which I diffolved in boiling water, and faturated with the alkali; and by evaporating obtained a beautiful pure white neutral falt, which is reprefented by fig. 8; *a. a.* fhews a part of the cryftallifation where the falt fhot into longifh cryftals, refembling fomewhat the fmall ones of nitre; none of them exceeded the length of half an inch; *b. b.* other pieces of the cryftallifation, which had a different appearance; *c. c.* a piece where it appeared like a fmall granulated falt; *d. d.* fome fmall roundifh or fquare cryftals, which adhered like a cruft

a cruſt to the ſides of the tea-cup; *e. e. e.* detached cryſtals.

EXPERIMENT IX.

With the acid of tamarinds.

Having had a preſent of ſome tamarinds in pods, from Mr. Arch. Gloſter, practitioner in phyſick in Antigua, I took out the pulp, and put about two pound of it into three quarts of water; and then ſaturated its acid with the alkali, and, after filtrating the liquor, I evaporated it to the conſiſtence of a ſyrup, and then put it into a cool place for 24 hours; when I found that a cryſtalliſation had actually taken place, I ſeparated the ſalt from a thick ſweetiſh liquor of the conſiſtence of a ſyrup; after it was dried, it had the appearance of a piece of common moſs, made up of a number of ſmall cryſtals diſpoſed in an irregular manner, and mixed with viſcid or ſaccharine juices. The letters *a. a.* &c. of fig. 9. ſhew ſome pieces of this ſalt while it remained in this form.

As I ſuſpected this ſalt to be ſtill mixed with a viſcid matter, I diſſolved ſome of it in warm water, and cryſtalliſed it anew, when it had a very different appearance, for it had ſhot into an infinite number of very ſmall cryſtals, which came every where from centres. The length of theſe cryſtals did not exceed half an inch at moſt; they were no thicker than horſehairs, or common white thread. How many cryſtals ſhot from each centre I could not determine; but, in many places, the cryſtalli-

ſation

ſation roſe into ſmall oblong, oval, or roundiſh tufts, made up of an infinite number of the ſmall cryſtals that ſhot from the centre towards the circumference.

Some of the concentrated liquor having been accidentally left in a ſaucer, and on the ſides of a tea cup at night, next morning the liquor in both veſſels had ſhot every where into ſmall fine cryſtals, that came like radii from a centre; in ſome places they had compleated the circle, in others only half, and in others only the two oppoſite quarters.

I treated three pound of Eaſt-India tamarinds, which I bought in a ſhop in Caſtle-Street, in the ſame manner; only, after they were ſaturated with the alkali, and the liquor filtrated, it was ſet by for ſome weeks, and then filtered again before it was evaporated. The people of whom I bought theſe tamarinds told me, that there was no ſugar mixed with them; and I believe what they ſaid was true, for I obtained eaſily a very pure and fine ſalt from them. Having at firſt carried the evaporation too far, as ſoon as the liquor was removed from the fire, it immediately began to concrete in form of a number of ſmall circles on the ſurface of the liquor, which were ſucceſſively ſucceeded by others, till the whole became one ſolid maſs; but on diſſolving this maſs in water, and evaporating only a little, and ſetting the liquor in a cool place to allow the ſalts to concrete, the cryſtalliſation began on the ſurface of the liquor, in form of ſmall circles or ſtars, and I obtained a ſalt in every reſpect ſimilar to the former.

The general appearance of this ſalt is repreſented by *b*, *b*, in fig. 9, and the different appearances in different parts of the cryſtalliſation by *c*, *c*.

Expe-

Experiment X.

With the acid of plums.

Having got a quantity of the larger ſort of the green plums, I cauſed the ſtones to be picked out, and the plums to be bruiſed and put into a large china bowl; I then poured about two quarts of water over them, and ſaturated their acid with the foſſil alkali; after they had ſtood 24 hours, I ſtrained off the liquor, filtered, and evaporated it, till there remained only a few ounces, when it was ſet in a cool place for four days, at the end of which time I found that a cryſtalliſation had taken place; but, upon pouring off the ſuperfluous liquor, I could not obſerve any general form of cryſtalliſation; the whole was made up of a number of very thin, flat, longiſh cryſtals, from about $\frac{1}{12}$ to $\frac{3}{4}$ of an inch long, and from $\frac{1}{12}$ to $\frac{1}{6}$ or $\frac{1}{5}$ of an inch broad, of an irregular figure, laid without any particular form, and mixed every where with a mucous and black oily matter; when dry, the whole appeared like a confuſed maſs, where however the form of ſome of the cryſtals was to be obſerved, as is to be ſeen at *a*, *a*, in fig. 10; in ſome places the cryſtals ſeemed to be laid with their edges uppermoſt, and in others in a different manner, as at *b*, *b*, &c.

In order to know the regular and true form of the cryſtalliſation of this ſalt, I ſeparated a quantity of the pureſt from the large maſs, diſſolved it in boiling water, filtered it through paper, and cryſtalliſed it a ſecond time in a tea cup. It now appeared in a more regular form; the cryſtalliſa-

tion

tion was divided into four roundiſh clumps, or cluſters, which were ſeparated or diſtinguiſhed from each other by a furrow. Each clump was made up of a number of very fine delicate plates laid edgeways, in ſomewhat of a regular manner; and between them a number of others, where part of the flat ſides were to be ſeen, and amongſt them an infinite number of ſmall rhomboidal or roundiſh cryſtals; the clumps appeared, in miniature, in the ſun, ſomewhat like to the lower part of the ſpread tail of a peacock. The letters *c*, *c*, *c*, *c*, ſhew the general form of the cryſtalliſation, *d*, *d*, *d*, the form and ſhape of ſome of the ſeparate cryſtals.

I treated another parcel of plums of the ſame kind in a different manner; I ſaturated part of the liquor when freſh, and let it ſtand till a fermentation had taken place before I evaporated it; and I let the other half ſtand till the fermentation was over before ſaturating it; but the ſalt obtained from both, appeared nearly in the ſame form, though the number of clumps in this ſecond cryſtalliſation was only three: and in a third experiment the appearances were exactly ſimilar, only the ſalt did not divide into clumps.

This ſalt taſtes cooliſh on the tongue, but does not affect the thermometer, in the time of its ſolution in water.

In the preſent hard froſt* ſome of the ſalt of plums, which had been diſſolved in three or four times its own weight of water, and ſet by in a cloſet, cryſtalliſed anew. The cryſtals were flat, thicker than a ſhilling, and moſt of them had ſix ſides of unequal

* N. B. The account of this laſt part of the experiment was given in to the Royal Society in the beginning of January 1768.

lengths,

lengths, as reprefented by *h*, *h*, in the figure marked large cryftals of neutral falt of plums: where they were run together, their figures were moft irregular as at *i*, *i*; fome few fmall ones were fquares, as at *k*, *k*. Hence we fee what a variety the different methods of cryftallifation make in the figures of thefe falts.

EXPERIMENT XI.

With the acid of mulberries.

Three quarts of the pure juice of the mulberry being faturated with four ounces of the foffil alkali, filtered, clarified with the whites of eggs and evaporated, yielded a faline matter, mixed with a quantity of a mucus and oil; which, on being purified as much as poffible, by lying on a fpongy brown paper, exhibited a very fine granulated falt almoft like fea fand, in which no regular-formed cryftals were to be obferved.

Some of this falt being again diffolved in water, evaporated and cryftallifed a fecond time, appeared in the tea cup like a cake made up of the fame fort of fine granulated falt as reprefented at *a*, *a*, fig. 11;—and another parcel *b*, *b*, which was treated in the fame manner, though it at firft appeared rougher, and more of a cryftalline form, yet on examination was found to be made up of the fame fort of faline matter.—On taking out the cake, there were a few thin very fmall fquare cryftals, fuch as thofe marked *c*, *c*, *c*, adhering to the bottom of the tea cup.

A fmall quantity of the faturated juice of the mulberries having been left by accident for ten or

twelve

twelve days in different tea cups and china bafons, there formed in each a number of figures, or fort of cryftals, refembling fomewhat the alphabet of the Chinefe language, interfperfed with a few fmall, oblong, parallelogram-fhaped cryftals, as at *d, d, d*.

Three quarts more being faturated with the alkali, were allowed to ftand for five or fix weeks, and then filtered and evaporated to about feven or eight ounces; the greater part of which was put into a ftone bafon, and about half an ounce into a fmall china bowl; after ten days, the liquor in the bowl had fhot into a number of fmall thin cryftals, fuch as reprefented at *e, e, e*;—and that in the ftone bafon into a fine pure falt made up of fimilar cryftals, but thicker and larger, fuch as thofe at *f, f, f*. —Thefe laft are certainly the true laft cryftals of this falt.

Experiment XII.

With the acid of grapes.

A bafket of grapes, which were brought to market for ripe, though many of them were ftill hard and four, and not come to their full perfection, yielded three quarts of juice, which I filtered and faturated with two ounces and a dram of the foffil alkali:—after it had ftood for a month to depurate, it was again filtered, and then clarified with the whites of eggs, and evaporated to about five ounces, when it became of the confiftence of a fyrup, and had fomewhat of a fweetifh tafte.

It was then fet in a cool place for two days to cryftallife; but, inftead of forming any regular cryftals, it concreted in form of a faline matter, refem-

refembling coarfe loaf fugar, when it firft concretes in a thick fyrup; fome of which being dried on brown fpongy paper, became very white, and feemed to be made up of long, very fmall cryftals, no thicker than human hairs, and a faccharine matter.

From this appearance, I judged that the falt was ftill mixed with a quantity of vifcid juices; and therefore I diluted the whole with a quart of New River water, depurated it again with the whites of eggs, and evaporated it to four ounces, which I fet in a cool place for eight days; and then, on examining, I found that a cryftallifation had taken place, and I obtained above a dram of a pure neutral falt, made up of fmall, fquare, and cubic, and fmall narrow oblong parallelogram cryftals, refembling fomewhat in appearance thofe got from the juice of the mulberry, only the cryftals were lefs, as may be feen in fig. 12.

After feparating this falt, I fet the remaining liquor again in a fand heat, and evaporated about half an ounce of it, and then put it for fome days into a cool place; and there formed a faline faccharine-like concretion, exactly fimilar to what was got in the firft trial.

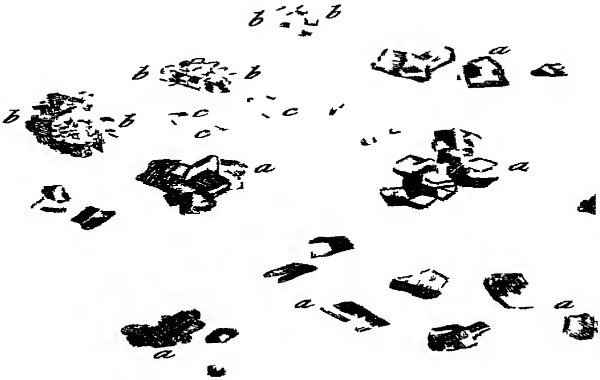
SECTION II.

Of neutral Salts formed with fermented vegetable Acids, and the foffil Alkali.

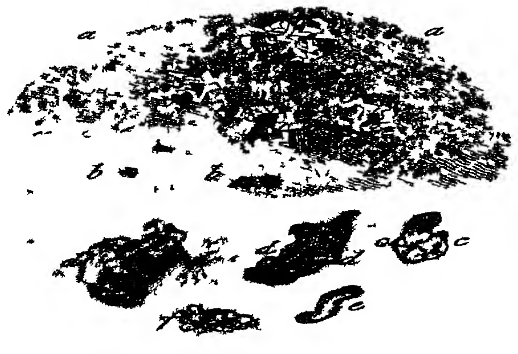
Having fhewn a variety of neutral falts made with native vegetable acids, we come next to take a view of thofe made with fermented acids, and fhall begin with that produced from vinegar.

EXPE-

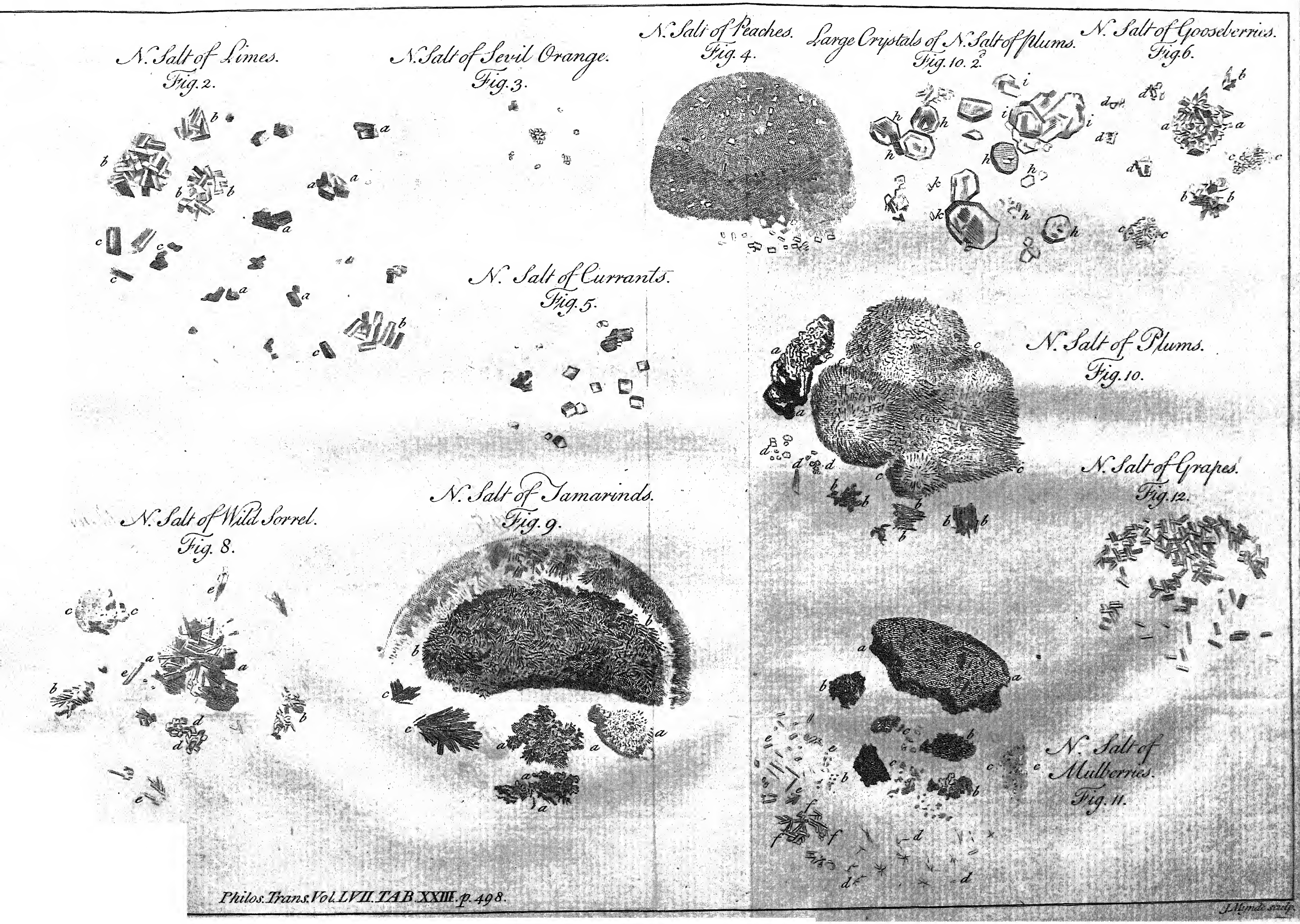
N. Salt of Lemons.
Fig 1.



N. Salt of Apples.
Fig. 7.



G. [illegible] delin.



Philos. Trans. Vol. LVII. TAB. XXIII. p. 498.

Experiments I. and II.

With common wine and diſtilled vinegar*.

The plain vinegar uſed, was ſaid to be the beſt white wine vinegar that could be got; and the diſtilled was ſaid to be prepared from wine vinegar likewiſe.

I ſaturated a pint of each of theſe vinegars, with the pure alkali, evaporated them to a pellicle, and let them ſtand to cryſtalliſe.

From the diſtilled vinegar I obtained the ſalt repreſented by *a*, *a*, *a*, fig. 14, Plate XXIV. which in the evaporating glaſs appeared as you ſee it, reſembling the figure of a ſun in a fire-work. Its cryſtals were a little twiſted; and upon taking them out, and examining them ſeparately, they appeared like ſo many ſmall cryſtals of glauber ſalt, as are repreſented by *b*, *b*, *b*.—On laying the evaporating glaſs on one ſide to allow the liquor to drain away, ſome thin flat ſquare cryſtals, ſuch as are repreſented by *c*, *c*, *c*, formed on the ſides of the glaſs.

And what is very particular with regard to this ſalt is, that, on diſſolving ſome of the cryſtals, reſembling thoſe of glauber ſalt, in pure water, and fully ſaturating the water with the ſalt, in ſome days there formed a number of cryſtals very different in figure and appearance from the former; be-

* The neutral ſalt with vinegar I find mentioned, but not deſcribed, in a chemical dictionary publiſhed at Paris in the year 1766.—All that is ſaid of it is, that it is a ſalt which cryſtalliſes eaſily, but is little known. See the articles Alkali Mineral, Sel neutre, and Vinaigre.

ing ſome of them ſquares, others longiſh parallelograms, others irregular pentagons, and ſome with ſix ſides; ſome were flat, and from $\frac{1}{12}$ to $\frac{1}{5}$, or $\frac{1}{4}$ of an inch thick; others appeared ſomewhat roundiſh, or oval, but the ſides were made up of flat ſurfaces, as are repreſented by the letters *d*, *d*, *d*, &c.

I do not remember to have ſeen ſuch a variety of ſhapes and figures in any other ſalt; and I cannot account for the great difference of appearance in the firſt and ſecond cryſtalliſation in any other way, than from the liquor in the firſt cryſtalliſation having been evaporated to a pellicle, and being very highly impregnated with the ſalt, the cryſtals began to ſhoot at once in every part of it, ſo that they had not room to extend in breadth, and to form themſelves into various ſhapes, as in the ſecond proceſs, where the liquor was not ſo highly concentrated, and where each cryſtal was formed ſeparate, and at a diſtance from another.

2. The plain wine vinegar, treated in the ſame way, but in a narrower veſſel, yielded a ſalt which had a different appearance from the former; for on pouring off the ſuperfluous liquor which remained after the cryſtalliſation was compleated, it ſeemed to be compoſed of a number of ſmall, thin, broad cryſtalline, ſquare plates, ſtanding up from about half an inch to an inch above the ſurface, as repreſented by *a*, *a*, *a*, in fig. 13; on ſeparating them, each cryſtal appeared at the baſis like a ſmall cryſtal of glauber ſalt, which terminated at the top in the thin broad plate already mentioned, as repreſented by the letters *b*, *b*, *b*, &c.

On diſſolving ſome of this ſalt in water, and letting it ſtand for 18 or 20 days, there formed a number

number of cryſtals of different ſhapes, as in the diſtilled vinegar; ſome ſuch as them, others reſembling exactly the figure of the Rochelle ſalts, but ſmaller as thoſe at *c x*; ſome ſquares, others of different ſhapes and figures, as at *c*, *c*, *c*, &c. and one large one marked *d*, *d*.

Both the ſalts from the diſtilled and from the plain wine vinegar, have a pleaſant cool taſte, without any diſagreeable bitter; and generate cold in the time of their ſolution in water, for the quickſilver in the thermometer, which ſtood at 63 in New River water, ſunk to 62, as ſoon as ſome of this ſalt, which was put into it, began to diſſolve.

From the figure and ſhape of ſome of the cryſtals of the ſalt of the wine vinegar coming ſo near to that of the Rochelle ſalt, I think we may reaſonably conclude that the acid of vinegar approaches near to that of tartar, but is not entirely the ſame.

The ſalt of the diſtilled vinegar can be made with great eaſe and very pure; but the common vinegar contains ſuch a large proportion of oil as to require ſome care to purify it after it is made.

EXPERIMENT III.

With the cryſtals of tartar.

The Rochelle ſalt, made with the acid of tartar, and the foſſil alkali, is ſo common a purging ſalt, that I ſhall not enter into any deſcription of it, and I have only given a figure of ſome of its cryſtals at fig. 15, that we might be able to compare it with

with the other neutral ſalts made with vegetable acids.

Experiment IV.

With the acid of verjuice of Apples.

I made two trials with the verjuice; the firſt was with a quart of what was old and tart, and took rather above an ounce of the alkali to ſaturate it; the other, which was newer, not ſo tart, and was ſaturated with about ſeven drams of the alkali.

The firſt was evaporated to a few ounces, when an oily or mucous pellicle appeared on the top; after letting it ſtand for ſome days, no ſalt was likely to cryſtalliſe; I therefore diluted it with water, filtered it, clarified it with the white of an egg, and evaporated it a ſecond time; and after it had ſtood for ſome days the ſalt concreted into the form repreſented by fig. 16. It was compoſed of a number of ſmall long cryſtals, which branched out from centres ſomewhat like the ſticks of a fan, or the fibres in the leaf of a tree, ſuch as at *a*, *a*, *a*.

The ſecond or new verjuice, after being ſaturated, was allowed to ſtand for four or five weeks, then filtered, purified with the white of an egg, and evaporated; and after ſtanding ſome days in a cool place, a cryſtalliſation was formed, which approached very near in its appearance to the ſalt of apples, though ſomewhat different; it was compoſed of a number of very fine delicate ſmall, flat ſquare or rhomboidal plates ſet upon their edges, near to one another, without any certain regular order

order that I could obſerve, but ſo as on the whole to make a very beautiful appearance; in this cryſtalliſation the ſalt ſeemed to form in clumps, two of which are to be ſeen at *b*, *b*, &c. and a profile view of a ſmall piece at *c*, *c*, and a figure of ſome of the plates laid on their flat ſides at *d*, *d*.

Did the difference of the age of the verjuice employed in theſe two experiments, or the difference of the proceſſes they underwent, make the difference in the appearance of the ſalts obtained in the different cryſtalliſations? The ſalt of the old verjuice approached to that of vinegar; of the new to that of apples.

Experiment V.

With the acid of perry.

At the time I gave in this paper, in the beginning of November, I had made ſeveral attempts to obtain a neutral ſalt from perry (or the fermented juice of pears) but without ſucceſs, owing to the large quantity of ſaccharine juice with which this liquor abounds. But having accidentally left ſome of the concentrated liquor in a ſmall china baſon, on examining it ſome days after the preſent hard froſt had begun, * I found that a cryſtalliſation had taken place.

The cryſtals were flat, long, narrow, very thin tranſparent plates, ſuch as repreſented in fig. 17; they were from a quarter of an inch to near an

* The account of this experiment was given to the Royal Society about the middle of January 1768.

inch

inch long; they were moſtly fixed to the ſides of the baſon by one end, many ſtood almoſt upright, and others lay acroſs each other. One end was commonly made up of two ſhort ſides, which met at a point.

They remained ſome days expoſed to the air in a cold room, and preſerved their tranſparency and figure; but after they had ſtood for about a quarter of an hour in a warm room, while the painter was drawing the figure, they loſt their tranſparency, and became white and mealy. They taſted cool, and ſomewhat bitter in the mouth.

SECTION III.

Of neutral Salts, formed with diſtilled vegetable Acids, and the foſſil Alkali.

Acids diſtilled from wood, and other vegetable ſubſtances, have been mentioned as a diſtinct ſpecies, but no proof has been brought of their differing from the other vegetable acids; on the contrary, in the tables of neutral ſalts given by chemiſts, no notice is taken of any neutral ſalts made with theſe acids; and therefore it is to be preſumed that they imagined them to be nearly of the ſame nature with the others.

In order to know if theſe acids differed from one another, and from the native and other acids, I had ſome guaiac wood, ſome fir wood, and ſome honey diſtilled, and procured ſome of the acid of each, which I ſaturated, filtered, evaporated, and cryſtalliſed.

EXPERIMENT I.

With the acid of guaiac wood.

The cryſtals of the neutral ſalt of guaic wood were long and ſmall, and ſhot like the rays of the ſun from a centre, and appeared as repreſented in fig. 18.

EXPERIMENT II.

With the acid of fir wood.

The neutral ſalt of fir had a very different appearance; there were no ſuch diſtinct cryſtals as in the other; what were to be obſerved ſeemed to be long and ſmall, to come in many places from points, and to go in ſomewhat of a circular manner, or to deſcribe a curve, and appeared as repreſented by fig. 19.

EXPERIMENT III.

With the acid of honey.

It has been a doubt among naturaliſts, whether honey ſhould be ranked among the vegetable or the animal ſubſtances. Moſt chemiſts ſeem to think it ſhould be ranked among the vegetable, and look upon it as made up principally of the juices of plants collected by the bees; but, however that matter may be, the following is an exact account of

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the neutral ſalt made with the acid obtained from this ſubſtance by diſtillation.

In order to procure this acid, I prevailed with Mr. Winter, brother-in-law to Mr. Heineken, apothecary, to diſtil four or five pounds of honey in a retort; at firſt he imagined that I only wanted the watry phlegm, which has been called by the name of the ſpirit of honey, and ſtopped the diſtillation before the acid came over; but having diſtilled a ſecond quantity, he procured me about ſix ounces of a very acid liquor, which I mixed with the phlegm or ſpirit which he firſt brought me; I then ſaturated the whole with the foſſil alkali, filtered and evaporated the liquor to a pellicle. After it had ſtood all night in a cool place, I found the pellicle to be compoſed of a yellow, bitter, ſaltiſh, mucous and oily matter; below which was a dark purpliſh liquor, which I poured into a tea cup, and there remained at the bottom of the ſtone gallypot, in which the evaporation had been performed, a yellow concreted matter, ſomewhat of the appearance of yellow wax, mixed with a little honey; on the ſurface of which was to be obſerved a number of globules of the ſame ſort of matter, of the ſize of muſtard ſeeds, and interſperſed with a black very bitter ſtuff. Next day, on examining the dark coloured purpliſh liquor which I had put into the tea cup, I found that a great part of it had concreted into a very beautiful ſalt, which is repreſented by fig. 20. *a, a, a,* ſhews the general form of the cryſtalliſation; *b, b, b, b,* the ſhape, figure, and ſize of ſome of the cryſtals. The cryſtals were almoſt all flat, and ſeemed in general to aſſume the form of long,

long, narrow parallelograms, or longiſh ſquares, if I may be allowed to uſe the expreſſion; *c*, *c*, ſome of the yellow ſaline matter.

This ſalt is pleaſant to the taſte, and evidently generates cold in the mouth in the time of its ſolution; but I had not quantity enough to try with a thermometer what degree of cold it generated in the time of its ſolution in water.

SECTION IV.

Of neutral Salts formed with Flowers of Benzoin, and Salt of Amber.

EXPERIMENTS I. and II.

With the flowers of benzoin.

Moſt modern chemiſts have looked upon the gum benzoin as a reſinous ſubſtance, which bears the ſame analogy to the vegetable reſins, as the ſuccinum or amber does to the foſſil bitumens; and they have eſteemed the flowers of benzoin to be an acid ſalt, mixed with an oily and a ſmall proportion of an earthy matter; but have brought no proof of its being ſo.

1. In order to aſcertain this fact, I put two drams and a half of the flowers of benzoin into ſome water, and then dropped into it by degrees a ſolution of the foſſil alkali; every drop raiſed an ebullition or effervéſcence, in the ſame manner as when any common alkaline ſalt is thrown into an acid

acid liquor. I continued adding the alkaline lye till all ebullition ceaſed, and the flowers were fully ſaturated and diſſolved; after which I filtered the liquor, and evaporated it till a pellicle began to appear, and then ſet it in a cool place all night, and next morning I had a fine pure tranſparent neutral ſalt, ſuch as is repreſented by figure 21. It adhered to the china baſon in form of a ſaline cruſt, which I removed; and on looking thorough it in the light, it ſeemed to be compoſed of an infinite number of very ſmall cryſtals; above this lay, in many places, a number of cryſtals of the figure of ſmall oblong parallelograms, as thoſe at *b*, *b*. But from the greater part of the ſurface of the cruſt there aroſe a number of very fine thin delicate plates of irregular figures, ſtanding on one edge; ſome were ſquares, others parallelograms, and others had more ſides, the general appearance of which was ſuch as is to be ſeen at the letters *a*, *a*, *a*, &c.

This ſalt, when firſt made, appeared as tranſparent and clear as glauber ſalt, or nitre; but on being expoſed to the air, became very ſoon white and mealy.

In the time of the evaporation of this ſalt, a ſaline white mealy cruſt roſe every where on the ſides of the china baſon in which the operation was performed, and even came over ſo far, as to cover its whole outſide. What roſe in this manner had a ſweetiſh taſte, and was not ſo ſharp in the mouth as what appeared in a tranſparent ſaline form.

The ſuperfluous liquor, which remained after the cryſtalliſation was compleated, being put into a tea

tea cup, concreted in a very uncommon manner. In the middle of the tea cup it arose something like a plant, or a fountain, where the water is discharged from a number of pipes, and spread from the bottom of this, so as to cover both the inside and outside of the cup, with a sweetish, white, mealy, saline crust, which in many places seemed disposed like the fine fibres of plants, or of the leaves of trees.

2. As a further proof of the flowers of benzoin being an acid of a particular kind, I saturated some of them with the sal volatile ammoniacum, evaporated and crystallised; and obtained an ammoniacal salt, which had a very singular appearance. It was covered on the top with a very white saline pellicle, below which were a number of thin, flat, white transparent crystals, the greater number of which seemed to be exact squares, some few, oblong parallelograms, such as are represented in fig. 22.

The flowers of benzoin generated a considerable degree of cold in the time of their saturation with the volatile alkali; they sunk the quicksilver in the thermometer from 52 to 46.

EXPERIMENTS III. and IV.

With the salt of amber.

The salt of amber is now generally known to be of an acid nature; but from what Mons. Bourdelin has said of it, in the Memoirs of the French Academy of sciences for the year 1742, its acid has

been looked upon by many chemists *, to be exactly of the same nature as the spirit of sea salt, only mixed with a little of the oleum succini;—though some have imagined it to be an acid of the vitriolic kind.

1st, When I first mixed this acid with the fossil alkali, I began to believe that what Monf. Bourdelin had alledged was true; for the liquor tasted saltish, like to a weak solution of sea salt in common water, but I was soon convinced of my error; for on evaporating and crystallising, I had a salt very different in its nature and properties from that of sea salt, or of glauber salt, one of which salts it must have been †, had the acid been the marine or the vitriolic. This agrees with what Dr. Stockar de Neuforn ‡, has said of this being a particular acid.

* Macquer seems to be thoroughly convinced of the acid of succinum, or amber, being the same with that of sea salt; for in mentioning the proofs which Monf. Bourdelin has brought of its being so, he says, "C'est ce point qui est l'objet principal de "memoire de Monf. Bourdelin; & cette decouverte est sans "contredit une des plus belles, & en même temps des plus dif-"ficiles, qu'il y eût à faire sur ce Bitume." See his Elemens de "Chymie pratique, tom. ii. p. 213.

† Sea salt is a neutral salt made of the fossil alkali, and marine acid, or spirit of sea salt; and glauber salt, of the same alkali and the spirit of vitriol.

‡ In the year 1760, Dr. Jo. Geo. Stockar de Neuforn, in his inaugural Dissertation de Succino, published at Leyden the 7th of July, 1760, proves by a number of experiments, that the acid of succinum is neither that of vitriol nor of sea salt; and he mentions two neutral salts made with this acid, the one with the common vegetable alkali, and the other with the volatile.

He says that the crystals of the one, made with the vegetable alkali, are clear and pellucid, and of the same figure as the

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The cryſtals, I obtained in the firſt experiment I made, were large and flat; and ſuch as are to be ſeen at the letters *a*, *a*, &c. fig. 23; they were of no certain ſhape or figure; ſome were roundiſh with a number of ſides, others appeared ſomewhat triangular, and others of different figures; and in ſome parts the cryſtalliſation appeared like a piece of rock work. I diſſolved ſome of this ſalt in water, and cryſtalliſed it a ſecond time, but the cryſtals were in general ſmaller than in the firſt operation; and the cryſtalliſation appeared as repreſented by *b*, *b*, *b*. In order to ſhew the difference between this and ſea ſalt, I made Mr. Paul draw the figure of ſome beautiful cryſtals of ſea ſalt, near to thoſe of this neutral ſalt of amber.

This ſalt is extremely different in its taſte from that of ſea ſalt, and certainly likewiſe in its virtues and properties.

2. In order to aſcertain more fully that the ſal ſuccini is an acid *ſui generis*, I ſaturated ſome of it with the volatile ammoniac ſalt, cryſtalliſed it, and obtained a neutral ammoniacal ſalt very different from that of the common ſal ammoniacum; it was compoſed of a number of ſmall long narrow flattiſh cryſtals, whoſe ſides were made up of four flat ſurfaces, ſuch as thoſe repreſented in fig. 24, and laid

cryſtals of the ſalt of amber itſelf; that it has a particular taſte, and diſſolves eaſily in water, which the tartarus vitriolatus does not; and when thrown on the fire, or put on a red hot iron, crackles and melts, but yet remains fixed and neuter. Acids make no change on it, nor is aqua fortis converted into an aqua regia by its mixture; it does not precipitate ſilver from ſpirit of nitre, though it precipitates lead from vinegar, in form of a white calx, which, however, cannot be changed into a ſaturnus corneus.

in

in an irregular order, ſome lying acroſs others, and ſome ſtanding on one end *.

The ſal ſuccini generates a great degree of cold in the time of its ſaturation with the volatile alkali, for it ſunk the quickſilver in the thermometer from 52 to 40; in this it agrees with the common ſal ammoniacum.

The Concluſion.

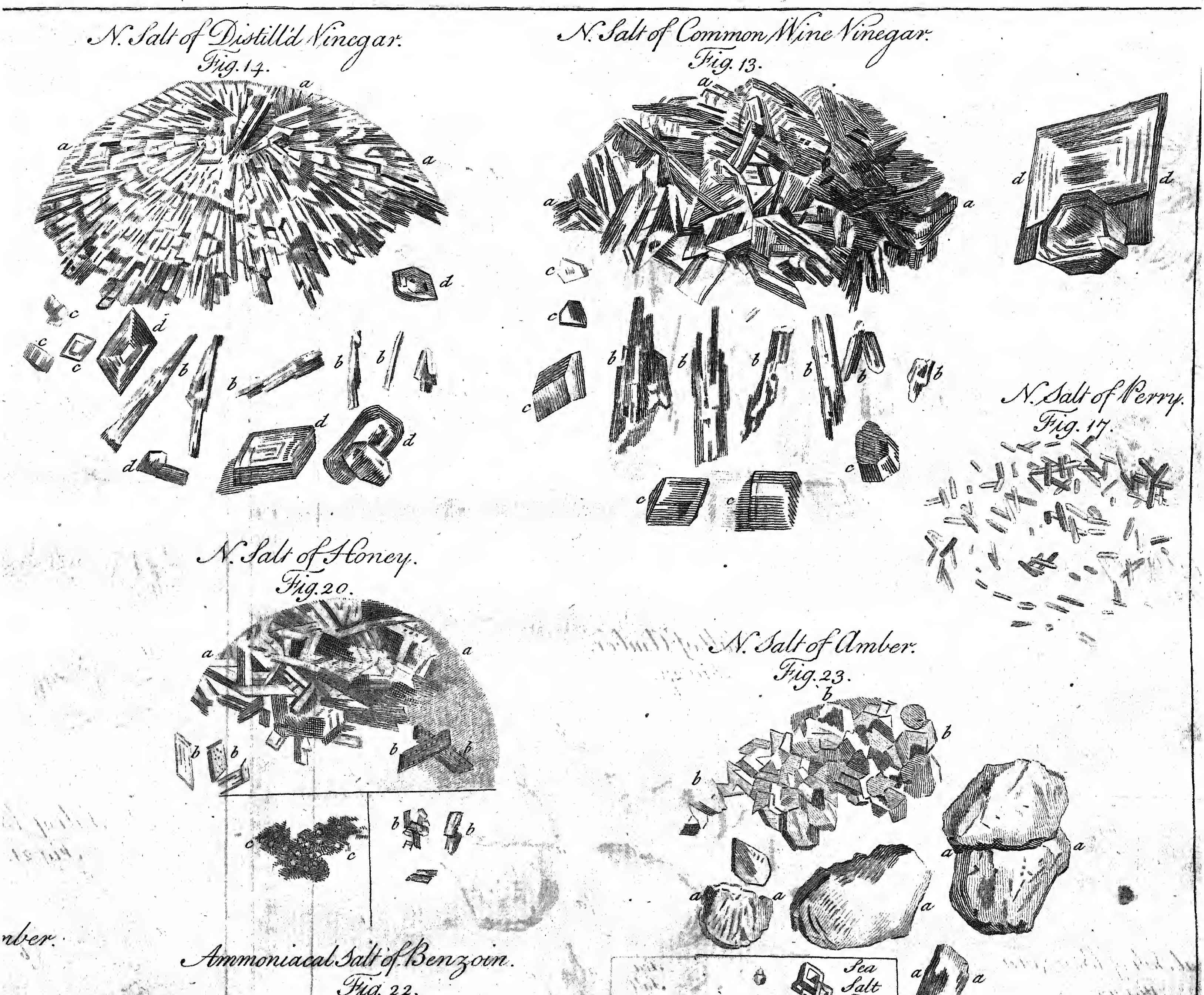
From the experiments above related, it is evident that phyſicians have hitherto been in a great miſtake, in believing that all vegetable acids were nearly of the ſame nature; for from them it ſhould ſeem that almoſt each of the acids, called vegetable, has ſomething peculiar to itſelf, and upon future trials may be found to have different virtues and properties †.

The different appearance of the neutral ſalts above mentioned, from that of thoſe produced by the union of the foſſil alkali with any of the mineral acids, ſeems to make it doubtful whether the vegetable acids derive their origin from the mineral,

* Dr. Stockar de Neuforn ſays, that this ammoniacal ſalt does not precipitate ſilver from aqua fortis, nor change aqua fortis into aqua regia; and when put in a ſilver ſpoon, and ſet over the fire, it melts and flies off in form of a vapour.

† However, it ought to be remarked, that when any of the concentrated ſaturated liquors ſtood for ten or twelve days before they cryſtalliſed, for the moſt part ſome cryſtals of a flat, ſquare, or of a narrow oblong parallelogram figure, were found adhering to the ſides of the cup or baſon in which the liquor ſtood; but whether this was owing to the alkaline baſis of theſe ſalts, or to the acids approaching to each other in their nature, is what can only be determined by future experiments.


N. Salt of Verjuice.
Fig.16.
Rochelle Salt.
Fig.15.
N. Salt of Guaiac Wood
Fig.18.
N. Salt of Fir.
Fig.19.
N. Salt of Benzoi
Fig.21.
Ammoniacal Salt of
Fig.24


N. Salt of Distill'd Vinegar.
Fig. 14.
N. Salt of Common Wine Vinegar.
Fig. 13.
N. Salt of Perry.
Fig. 17.
N. Salt of Honey.
Fig. 20.
N. Salt of Amber.
Fig. 23.
Sea Salt
Ammoniacal Salt of Benzoin.
Fig. 22.

ral; or whether they are not new ſubſtances, generated either in the veſſels of plants by means of the vegetative proceſs, or by fermentation, or by the force of fire. If they owe their origin to the mineral acids, they are certainly ſo much changed in their virtues, and properties by the combination of new particles, and by the proceſſes they have undergone, that they may be looked upon as diſtinct bodies in many reſpects.

From what has been ſaid, it is evident that the number of true neutral ſalts* is infinitely greater than what has been ſuppoſed, of late, by chemiſts; and it is probable that many of the neutral ſalts, above deſcribed, may prove to be excellent remedies in the cure of diſeaſes, as well as uſeful in many manufactories.

As there is ſuch a variety of vegetable acids, and as each of them produces a diſtinct neutral ſalt with each of the three alkalies, I think it would be right to diſtinguiſh them from one another by particular names; the ſalts made with the vegetable alkali may be called *vegetable ſalts*, as both the acid and the alkali are vegetable ſubſtances; thoſe made with the foſſil alkali *neutral ſalts*; and thoſe made with the volatile alkali *ammoniacal ſalts*, as all the neutral ſalts hitherto made with this alkali have gone by this name. Thus we may call the three neutral ſalts made with lemon juice: 1. Vegetable ſalt of lemons. 2. Neutral ſalt of lemons. 3. Ammoniacal ſalt of lemons.

* By true neutral ſalt is meant, a ſalt made with an acid and one of the three alkalies; the word *true* is added to theſe ſalts, to diſtinguiſh them from neutral ſalts, made with earths or metals, and acids.

By means of theſe neutral ſalts we may be enabled to diſcover many of the properties of vegetable acids, and particularly the different degrees of affinity or attraction between them and alkaline ſalts; thus, for example, if we diſſolve in water ſome of the neutral ſalt of currants, and add ſome lime juice or ſome vinegar, and then evaporate and cryſtallize: if we obtain a neutral ſalt of currants, we conclude that the acid of currants has a greater affinity or attraction to the alkali than the acid of limes or of vinegar; but, if we get a neutral ſalt of limes, or of vinegar, we conclude that theſe acids have a greater affinity with the alkali than the juice of currants.

As I am ſenſible that this account of vegetable neutral ſalts is very incomplete, and that I have done little more than given a very ſuperficial deſcription of their external appearance; and as it will probably require a length of time, and the labours of many, to diſcover fully their virtues and properties, I ſhall recommend it to thoſe who may proſecute this ſubject to endeavour to aſcertain the following facts:

1. What degree of cold or of heat is generated on the mixture of each acid with the different alkaline ſalts; and likewiſe to try the ſame experiment with each neutral ſalt at the time of its ſolution in water.

2. What quantity of pure alkaline ſalt it takes to ſaturate any determined quantity of each of the vegetable acids.

3. What figure each neutral ſalt aſſumes when it is firſt cryſtalliſed, and likewiſe after it has been

purified;

purified, and again diſſolved in water and cryſtal-
liſed.

4. What quantity of water it takes to diſſolve any determined quantity of each ſalt.

5. What effects theſe ſalts or their ſolutions in water have on oils, ſulphur, ardent ſpirits, metals, earths, and other ſubſtances; what ſubſtances they mix eaſily with, and to what bodies they prove a menſtruum, or aſſiſt in diſſolving.

6. How far they agree in their virtues and properties with the neutral ſalts made with mineral acids, and with each other.

7. What effects they have on the human body; whether they promote more particularly the perſpiration or the ſecretion by the kidneys, or whether they act more readily on the bowels, and promote the diſcharge by ſtool; and to aſcertain the exact and proper doſes of each.

8. And laſtly, what effects fermentation and diſtillation have on native vegetable acids; and to obſerve and compare the appearances of the neutral ſalts made with theſe acids in their different ſtates: viz. 1. In their native ſtate. 2. When made into wine. And 3dly, when made into vinegar; and likewiſe when made with acids brought over by the force of fire, or diſtilled from the ſame juices in each of the three different ſtates mentioned.

And in order to facilitate their labours, I ſhall conclude this long paper with obſerving,

1ſt, That all vegetable juices uſed for making neutral ſalts ought to be ſtrained through a cloth, and then filtered through paper, before they are ſaturated with the alkaline ſalt; and that, after they

are ſaturated, they ought to be allowed to ſtand for ſome days, and ſome of them for weeks, and then be filtered again, before they are evaporated.

2dly, That it is of uſe to clarify many of theſe juices, after being ſaturated, with the whites of eggs.

3dly, That it is ſometimes eaſier to obtain a neutral ſalt, by evaporating with a boiling heat, than with a ſlow or gentle fire; as the heat of boiling water coagulates, and throws up a quantity of viſcid juices to the ſurface, which cannot be eaſily ſeparated by any other means.

4thly, That the ſweeter any fruit is, and the more it abounds with ſaccharine or viſcid juices, the more difficult it is to obtain a neutral ſalt; and for this reaſon I have not hitherto been able to get any neutral ſalt from the ſaturated juices of pears, or of cherries.

5thly, That, in caſes where we are obliged to employ water mixed with the fruits cut ſmall, inſtead of their juices, it is right to peel off the ſkins before we attempt to ſaturate the acid; otherwiſe the alkaline ſalt is in danger of uniting with, and rendering ſoluble in water, the groſs oils with which the ſkins generally abound, which afterwards prevent the cryſtalliſation of the neutral ſalts.

Received

Received November 19, 1767.

L. *Experiments on the Diſtillation of Acids, volatile Alkalies, &c. ſhewing how they may be condenſed without Loſs, and how thereby we may avoid diſagreeable and noxious Fumes: In a Letter from Mr.* Peter Woulfe, *F. R. S. to* John Ellis, *Eſq; F. R. S.*

Title read December 17, 1767.

SIR,

Read Feb. 4, 1768.

IN the common manner of diſtillation there eſcapes a great quantity of fumes, which cannot be condenſed; and in ſeveral operations theſe fumes are very hurtful to the lungs. By the following method of diſtillation theſe fumes are totally condenſed, which makes a great ſaving in ſome diſtillations, and the operator is in no danger of being hurt by any pernicious vapours.

This new method conſiſts in making the fumes paſs by a ſmall glaſs tube through water, which hereby becomes charged with the vapours, that would otherwiſe eſcape.

Deſcription

Defcription of the apparatus, TAB. XXV.

Fig. 1.

A a retort.

B a receiver, with a fpout at bottom, for the diftilling liquor to run into the bottle C; the recipient has alfo a fmall opening on one fide at D.

E a crooked tube $\frac{1}{10}$ and $\frac{1}{2}$ of an inch bore.

F a veffel containing water.

The crooked tube E is fitted to the fpout D of the receiver by means of a cork with a hole in its middle, and then well covered with lute; the other end of it goes to the bottom of the veffel F, to the mouth of which it is fitted by a cork, with a femicircular notch in it as at G, but without any lute to faften it, as there muft be a fmall vent for the efcape of the elaftic air, and this is the only vent in all the apparatus for that purpofe. By this apparatus the fumes are obliged to pafs through the water in F, and there depofit all they contain, except their elaftic air.

In moft diftillations there is a quantity of air abforbed at different times during the procefs; and in this cafe the external air would prefs on the water at F, and force it by the tube into the veffel C, which might fpoil the diftilled liquor. This may be prevented by letting air into the receiver or bottle C, by boring a hole through the lute; this however may be inconvenient, on account of the conftant attendance which is neceffary; but the following apparatus will prevent it. See figure 2. It confifts

consists in fitting an empty vessel H, to the apparatus described before, See figure 1. By this means the water is forced into H, and by the stopper at L it may be emptied, and put back into the vessel F, the crooked tubes D and I are fitted to H, by a cork with two opposite semicircular notches as at K, and then well covered with lute.

Experiment I.

On the distillation of sal ammoniac with quick lime.

12 ℔.* of British sal ammoniac, and 26 ℔ of quick lime were powdered, mixed, and put into the iron body A (fig. 3.); and when the apparatus † was luted, a gallon of water was poured on it through the orifice (b), which was immediately stopped; the lime growing hot produced a vast quantity of elastic air, which though highly charged with volatile alkaly was condensed by the water in F, fig. 2. the air only escaping at the top of this vessel with hardly any sensible volatile alkaline smell. Next morning, all being cold, another gallon of water was added as before, and a very slow fire made under the body for 14 hours, in which time there distilled near a pound of volatile alkaly; the fire was then made stronger, and continued in that state for twelve hours more, in which time there was obtained, together with what was first distilled, 8 ℔ $\frac{1}{4}$ of volatile alkaly, strong and fit for Eau de luce; this was taken out of the bottle and set apart. The vessels being cool,

* In all the experiments averdupois weight was made use of.

† The spout of the stone head belonging to the body A, figure 3, is to be luted to the receiver B, figure 2.

two

two gallons more of water were put into the body, and the fire made as before, and continued till there was 7 ℔ diſtilled of weak volatile ſpirit; this anſwers better than water for a freſh diſtillation of ſal ammoniac and lime.

During the firſt 16 hours of the diſtillation, there continually eſcaped through the water of F elaſtic air very ſlightly charged with volatile alkaly, eſpecially when the water grew hot; but during the remaining time of the diſtillation, no elaſtic air was ſet free.

Two ſtone gallon bottles, with three quarts of water in each, were made uſe of to condenſe the vapours; and when one bottle was grown warm by the fumes, the other was put in its place, while it was a cooling in a veſſel of cold water; and ſo continually changed during the whole operation. The ſix quarts of water encreaſed by this means 2 ℔ and ½ in weight; and, by the following experiments it appears, that a pound of this vapour condenſed in the water is to a pound of the volatile alkaly, which was ſet apart for Eau de luce, as 140 to 76, which is nearly twice as ſtrong; therefore there was a ſaving of near [illegible] ℔ of volatile alkaly, which would have been loſt in the common manner of diſtillation.

The water of the two ſtone bottles charged with alkaline vapours was mixed, in order to reduce them to the ſame degree of ſtrength, and as much of it was put into a glaſs cucurbit as contained four ounces of the alkaline vapour; four ounces of the volatile alkaly, which was ſet apart for Eau de luce was put into another cucurbit of the ſame ſize, and

and diluted with water to the ſame volume of the other.

This laſt took 1 ℔ 3 ℥ of acid of vitriol, diluted with water, to be ſaturated, and did not grow hot; wheras the water containing the four ounces of alkaline vapours took up 2 ℔ 3 ℥ of the ſame acid of vitriol, and grew ſo very hot, that the veſſel could ſcarce be held in the hand, even after having been diluted at different times with two quarts of water. This ſhews that there is a great difference in the two, and that it is not intirely owing to ſtrength.

The heat produced by the vapours paſſing through the water, was tried at another diſtillation, and raiſed the quickſilver in Fahrenheit's thermometer to 110 degrees.

In rectifying cauſtic volatile alkaly with lime, there is likewiſe a very great quantity of elaſtic air ſet free, highly charged with volatile alkaly, which condenſes in water and heats it.

Water may be ſo ſtrongly charged with this vapour, that it will make very ſtrong Eau de luce, nay, much ſtronger than that which we ſaid before was diſtilled and ſet apart for Eau de luce: but it is neceſſary, as mentioned before, to make uſe of two ſtone bottles, changing them as often as they grow warm.

Experiment II.

On the diſtillation of the acid of ſalt by means of the acid of vitriol; for the apparatus ſee fig. 2.

* A green quart retort coated with loam was made uſe of for this experiment; and it was placed

* What goes by the name of a quart retort holds better than two gallons of water.

 in

in a reverberatory furnace on a naked fire; 14 ℔ of common ſalt was put into it, and on that the like quantity of oil of vitriol, which had been diluted the day before with 7 ℔ of water; the retort was then immediately luted to the recipient, and the diſtillation conducted in the common manner: the operation continued 16 hours, when hardly any more liquor would come over with a ſtrong fire.

To condenſe the vapours, two ſtone gallon bottles with three quarts of water in each were made uſe of, as in the former experiment.

In this operation there was obtained 9 ℔ 5 ℥ and $\frac{1}{2}$ of ſpirit of ſalt, which dropped into the bottle C; the ſix quarts of water in the ſtone bottles increaſed in weight 6 ℔ 12 ℥ and $\frac{1}{2}$; the caput mortuum weight 18 ℔ 6 ℥; ſo that in this operation there was only a loſs of eight ounces, which is but $\frac{1}{70}$ part of the whole, which probably was moſtly elaſtic air.

EXPERIMENT III.

The ſame operation was repeated with a ſlower fire, which continued for 23 hours, after which time hardly any more liquor would come over with a ſtrong fire.

There were here produced 11 ℔ 10 ℥ of ſpirit of ſalt, in the bottle C; the ſix quarts of water increaſed in weight 3 ℔ 10 ℥, and the caput mortuum weighed 19 ℔ 4 ℥; the loſs was the ſame as in the foregoing experiment.

In order to know the different degrees of ſtrength of the acids produced in theſe two experiments, they

they were ſaturated with a fixed alkaly diſſolved in water.

Four ounces of the acid in experiment II, which diſtilled into C, took of the alkaline liquor to be ſaturated 13 ℥ 5 ʒ 2 ℈.

As much of the water * in experiment II, as contained 4 ℥ of vapour took to be ſaturated 1 ℔ 9 ℥.

Four ounces of the acid in experiment III, which dropped into C, took of the ſame alkaline liquor to be ſaturated 12 ℥ $\frac{1}{2}$.

As much of the water of experiment III, as contained; four ounces of vapour † took to be ſaturated 2 ℔ 6 ℥.

Four ounces of oil of vitriol, which was to water in weight as 24 to 13, took of the ſame alkaline liquor to ſaturate it 1 ℔ 10 ℥ 7 ʒ, which ſhews that oil of vitriol is not ſo ſtrong an acid as the vapour of ſpirit of ſalt, when condenſed in water and diſtilled ſlowly, as in experiment III.

From the foregoing experiments it appears, that 1 ℔ of the ſpirit of ſalt vapour, condenſed in the water in experiment II, is to 1 ℔ of the acid of ſalt, which dropped into C of the ſame experiment, as 200 is to 109, which is near double; and therefore the 6 ℔ 12 ℥ and $\frac{1}{2}$ of the vapour, which condenſed in the water, is equal very nearly to 13 ℔ 1 ℥ of the acid which is diſtilled in C: ſo that by this method of diſtillation, this great proportion of acid is ſaved, and thoſe diſagreeable ſuffocating fumes avoided.

* The water of the two bottles was mixed together; for they were of different ſtrength.

† The water of theſe two bottles were likewiſe mixed together for the ſame reaſon.

 In

In experiment III, 1 ℔ of the acid vapours, which condensed in the water is to 1 ℔ of the acid of salt which dropped into C, as 131 is to 50, or as 2 $\frac{31}{50}$ to 1; and therefore the 3 ℔ 10 ℥ of acid vapours, which condensed in the water, is almost equal to 9 ℔ and $\frac{1}{2}$ of what distilled into C.

It further appears, that the slower the distillation is conducted, the more concentrated are the acid vapours that condense in water. In order to see whether there was any difference in the strength of the acid vapours, which were condensed in the water from the first to the last of the distillation, the following experiments were made.

Five pound of common salt, with 5 ℔ of oil of vitriol were distilled in a tubulated retort, and three bottles with an equal quantity of water in each were made use of to condense the vapours.

The first bottle increased in weight 3 ℥, and during this time, which was twelve hours, there was no fire under the retort; that bottle being taken away, another bottle put under, a fire was made; this bottle increased in weight 1 ℔ and half an ounce, the third bottle increased 10 ℥ and a half.

As much water of each of the three bottles as contained one ounce and a half of the acid fumes was saturated with an alkaly dissolved in water.

The water of the first bottle took to be saturated	11 ℥ $\frac{1}{2}$ a dram.
The second bottle took up	10 ℥ 2 ʒ 2 ℈
The third bottle	10 ℥ 1 ʒ
An ounce and half of oil of vitriol, which was to water, as 226 to 118 nearly, took up of the same alkaly	7 ℥ 6 ʒ

By which it appears that the fumes, which firſt aroſe without fire, are ſtronger than the ſecond, and the ſecond than the third.

It appears further, that the moſt concentrated portion of the acid of ſea ſalt is the moſt volatile, and that in ſtrength it is to the oil of vitriol mentioned before, as $44\frac{1}{4}$ to 31.

* In order to try the purity of the acid vapours, which were condenſed in the water, and of the acid, which diſtilled into the bottle C, the following experiments were made, and are marked a, b, c, d.

(a) Four ounces of the ſpirit of ſalt of the 2d experiment, was perfectly ſaturated with 4 ʒ of whiting.

(b) Four ounces of the ſpirit of ſalt of the 3d experiment, was perfectly ſaturated with 4 ℥ of ditto.

(c) As much water as contained 4 ℥ of vapour of the 2d experiment, was ſaturated with 5 ℥ of ditto.

(d) As much water as contained 4 ℥ of vapour of the 3d experiment, was ſaturated with 6 ℥ of ditto.

The reaſon of uſing more whiting with ſome than with others, was on account of the different ſtrength of the acids; and as there was a greater quantity of whiting than neceſſary uſed in theſe experiments to ſaturate the acids, the undiſſolved

* This depends on the property of the acid of vitriol, and the acid of ſea ſalt, combined with a calcareous earth; for this earth, combined with the acid of ſea ſalt, forms a very ſoluble ſubſtance; whereas the ſame earth, with the acid of vitriol, forms a ſubſtance inſoluble (or almoſt ſo), called ſelenite.

part

part muſt conſiſt of whiting; and, if any acid of vitriol in the acids, of whiting and ſelenite.

In order to ſeparate the ſelenite from the whiting, a large portion of diſtilled vinegar was made uſe of, which diſſolves the whiting, it being a calcareous earth; and in order to promote the ſolution, heat was made uſe of.

The undiſſolved part of (a) being perfectly ſaturated with a ſufficient quantity of diſtilled vinegar, and afterwards repeatedly waſhed with pure water, and dried, weighed $\frac{1}{2}$ oz. and 26 gr.

(b) treated as (a) weighed $\frac{1}{2}$ oz. and 52 gr.

(c) treated as (a) weighed 39 gr.

(d) treated as (a) weighed 42 gr.

* One ounce of whiting treated as (a) left 7 gr.

From theſe experiments it appears, that the

Four ounces of acid marked (a) contain as much acid of vitriol as will make

$\frac{1}{2}$ an oz. leſs 2 gr. of ſelenite.

Four ounces of acid marked (b)

$\frac{1}{2}$ an oz. and 24 gr. of ditto.

Four ounces of the acid vapour marked (c)

4 gr. of ditto.

Four ounces of the acid vapour marked (d) none.

Hence it is evident, that the vapour of the acid of ſalt condenſed in water, when diſtilled ſlow, contains no acid of vitriol; and that even when it is diſtilled quick, it contains ſo ſmall a quantity as is not worth notice.

* As whiting contains ſome parts which are not ſoluble in diſtilled vinegar, it was neceſſary to know how much of this an ounce contained, which muſt be deducted in proportion to the quantity uſed for the experiments a, b, c, and d.

If 10 ℥ of ſea ſalt are diſtilled in the common manner with an equal quantity of oil of vitriol unmixed with water, there only diſtil 2 ℥ of ſpirit of ſalt; whereas, if diſtilled in this new manner, we not only obtain the like quantity, but likewiſe 4 ℥ and ½ half more, which are condenſed in the water; ſo that in making this concentrated ſpirit of ſea ſalt, there is a ſaving of above double the quantity, which would be loſt in the common method of operating.

Of the heat produced by the vapours of ſpirit of ſalt paſſing through water, ſpirits of wine, and oil of turpentine:

Three quarts of water were put into a gallon ſtone bottle, and made uſe of to condenſe the vapours, as in experiment the 2d, fig. F; in three hours and a half after the fire was made under the retort, the water in the ſtone bottle had acquired the degree of 212, which is the mark of boiling water in Fahrenheit's thermometer; and at this time there was ſcarcely 2 ℥ of ſpirit of ſalt diſtilled into the bottle, fig. C. The receiver and bottle C ſeemed cold to the touch; the water at F had increaſed 2 ℔ 3 ℥. Another like bottle with the ſame quantity of water being put in the room of this, in ſome time, acquired the ſame degree of heat. The fumes ſeem to condenſe very well until the water acquired a heat within twelve degrees of boiling water.

Spirit of wine rectified, made uſe of inſtead of water to condenſe the vapours, acquires a heat equal to 188 degrees; and it grows of a deep brown colour, though tranſparent.

Oil

Oil of turpentine applied to the ſame uſe acquires a heat of 12 degrees above that of boiling water, or 224 degrees; it becomes of a dark brown colour, though tranſparent, and has a diſagreeable bituminous ſmell. The thermometer not meaſuring more than 213 degrees, could not be left in with ſafety any longer.

Another time oil of turpentine was made uſe of to condenſe the vapours, which proceeded from 1 ℔ $\frac{1}{2}$ of ſal ammoniac, with 1 ℔ $\frac{1}{2}$ of oil of vitriol, and $\frac{3}{4}$ of a pound of water: here it did not grow near ſo hot, nor ſo high coloured, as in the other experiment, but was for the moſt part congealed.

The difference of theſe two experiments may, perhaps, be owing to the ſmallneſs of the quantity of the ingredients in the laſt proceſs; for in the firſt there was 14 ℔ of ſalt, 14 ℔ of oil of vitriol, and 7 ℔ of water.

Of the re-abſorbtion of Air in Diſtillations.

In all diſtillations a quantity of elaſtic air is ſet free in the beginning, but afterwards there is a re-abſorbtion of the ſame; the following experiment was made to ſhew how great it is in ſome caſes.

For the apparatus, ſee fig. 1.

One pound and a half of foreign ſal ammoniac was put into a retort, and 1 ℔ $\frac{1}{2}$ of oil of vitriol (previouſly diluted the day before with $\frac{3}{4}$ of a pound of water) poured on it, and a recipient well luted to it; the recipient had a tube 31 inches, well fitted and luted to it; and this tube was immerſed in a glaſs veſſel containing a quart of water.

The

	lb	℥	ʒ
The spirit of salt which was distilled, weighed	1	2	5
The quart of water increased in weight		5	4
The caput mortuum weigh'd	2	3	4
The loss in the operation was only			3
	3	12	

The operation was continued till the sal ammoniac began to sublime.

When no more air escaped, which might easily be perceived by its ceasing to bubble through the water, the vessel of water was taken away, and the tube was immersed in a bason of quicksilver; the mercury rose in the tube 23 inches and a half, whilst the recipient was too hot to bear one's hand on it longer than half a minute; when the recipient was quite cold, the mercury rose to 29 inches and $\frac{1}{10}$, and there was near one inch of spirit of salt on its surface. This experiment was tried the 11th of November, when the barometer was at 30 inches. In order to make this experiment succeed, it is of the utmost consequence to lute well the vessels.

On the Marine Æther.

The Marquis De Courtenveau, of the Royal Academy of Sciences of Paris, has published a very curious memoir in their Transactions, on the making of Marine Æther, by distilling spirit of wine with the * liquor fumans of Libavius; but no one, that I know of, has succeeded in making it with the pure spirit of salt. It was natural to conclude

* The liquor fumans is made by distilling mercury sublimate with tin, and is composed of the acid of salt united with tin.

 from

from the extreme great acidity of the fumes of ſpirit of ſalt, that Æther might be made by ſaturating rectified ſpirits of wine with them; and on trial I found it anſwer, though not in a large quantity.

The ſpirit of wine, charged with the acid vapours, muſt be diſtilled and cohabated, and then rectified with a ſlow degree of fire *.

The method that Monſ. Beaumé of Paris propoſed to make this Æther, and which did not ſucceed with him on account of his not being able to condenſe the fumes, anſwered well with me; and it conſiſts of combining the vapours of ſpirit of ſalt with thoſe of ſpirit of wine. The apparatus that I made uſe of for this purpoſe is deſcribed at fig. 4, and the proceſs is as follows:

Eight pound of ſea ſalt was put into the retort B, and two quarts of rectified ſpirit of wine into the retort D; three pints of the ſame ſpirits of wine were put into each of the glaſs veſſels I and K, in order to condenſe the fumes, one not being ſufficient; all being well luted and ſecured, the ſpirits of wine in D were made to boil, and then 7 ℔ of oil of vitriol was poured on the ſalt in the retort B, at ten or twelve different times, ſeven minutes between each time, leſt the mixture ſhould boil over; then a fire was made under this retort, and both fires kept up till the operation was over. The quantity of liquor in the veſſels I and K, increaſes conſiderably from the vapours that condenſe therein; and the veſſel I in particular grows very hot, and being

* As I have ſhewn before, that the vapours of the acid of ſalt, which condenſe in water, are free from the acid of vitriol, we may be certain, that the acid of vitriol did not contribute to form this Æther.

highly

highly charged with vapour is rendered incapable of condenſing any more; the vapours then paſs on to the veſſel K, and heat that alſo.

The liquor then that was diſtilled into the veſſel F, was mixed with the liquor of the veſſels I and K, then being * diſtilled, cohabated and rectified ſlowly with ſlacked lime, produced a very ſubtile penetrating Æther; it is very remarkable, that this, though free from acid, upon mixing it with water, cauſed a violent ebullition.

An expeditious method of making Nitrous Æther by Diſtillation, without Fire.

(See fig. 5.) Pour ſix ounces of the moſt concentrated ſpirit of nitre, little by little, on eight ounces of rectified ſpirit of wine, ſhaking the veſſel each time in which the mixture is made.

Then convey it by a long funnel through the opening of the head at C, into the matraſs A; the opening is afterwards ſecured by a glaſs ſtopper; in warm weather this mixture grows hot in five or ſix minutes, and diſtills in a ſtream into the veſſel E, and is over in about half an hour. Before the matraſs grows cold, a freſh mixture is put in as above, and ſo on for five or ſix times, till there is liquor enough diſtilled. This liquor being ſlowly rectified

* Spirits of wine was uſed likewiſe here to condenſe the vapours; and though the diſtillation was conducted with a very ſlow fire, yet the ſpirits of wine grew very hot. Spirit of wine was likewiſe uſed to condenſe the vapours in the cohabation, but they did not grow hot.

This liquor without cohabation affords Æther, but not ſo great a quantity.

with ſlacked lime, makes very fine Æther. The ſpirit of wine, which was put into the veſſels E and F to condenſe the vapours, is ſo highly charged with Æther, that it will ſeparate on waſhing with water. This ſpirit of wine is alſo an exceeding rich ſpiritus nitri dulcis.

What remains in the matraſs contains a quantity of ſpirits of wine, which may be ſeparated by diſtillation.

On the Diſtillation of the Nitrous Acid, ſee fig. 2.

The quantity that is condenſed in water during the diſtillation of this acid ſpirit is ſo ſmall, that it would be ſcarce worth ſaving, if it was not to prevent thoſe noxious fumes, which have ſuch an effect on the lungs of the operator, as frequently to make him ſpit blood.

Water highly charged with theſe fumes by repeated diſtillations becomes blue, and retains its colour *.

I once diſtilled, in an iron body with a ſtone head, 30 ℔ of nitre, with 60 ℔ of green vitriol, which I had calcined to whiteneſs, and was obliged to make uſe of two veſſels of water, as in fig. 5, at F and G, to condenſe the vapours: this water became

* Oil of vitriol was uſed in this operation, to ſet free the acid of nitre; and I found upon trial the fumes condenſed in the water to be a pure ſpirit of nitre: whereas, in the other operation, where calcined vitriol or copperas was uſed, the fumes contained ſome acid of ſalt. This led me to try the common green copperas, and I found it contained a portion of iron united to the acid of ſalt: whereas the Dantzick copperas or vitriol contains no acid of ſalt, and therefore is fitter to make an aqua fortis for the refiners uſe.

blue in one diſtillation, and continued ſo for 18 months, till I made uſe of it.

A great quantity of air was ſet free from the beginning to the end of the diſtillation, owing in a great meaſure to the acid fumes acting on the iron body; for if diſtilled in a glaſs or ſtone veſſel, the quantity of air is not near ſo conſiderable.

The nitrous fumes condenſed in water, in making the ſpiritus nitri fortis appear to be more acid than the ſtrongeſt oil of vitriol made uſe of for the experiments on ſpirit of ſalt.

Water is not heated by theſe fumes, owing probably to the ſmalleſt of the quantity which condenſes therein.

A further application of this new method of Diſtillation.

In the diſtillation of the oil of vitriol, a great part of the acid comes over ſulphureous, and is very hard to condenſe; but, by paſſing it through water, this condenſation is eaſily obtained: however, a greater quantity of water is neceſſary for this operation than for the ſpirit of ſalt, though the water becomes but ſlightly acid, yet it is greatly ſulphureous, and at the ſame time acquires no heat.

The ſulphureous acid obtained by other means, as by diſtilling the acid of vitriol with mercury, and other ſubſtances, is likewiſe condenſible.

Further, this ſulphureous acid of vitriol may, by two or three ſlow rectifications, be deprived of its acid; but it will ſtill retain its penetrating ſulphureous gas-like ſmell.

The

The vapours which ariſe in the deflagration of nitre, with charcoal, antimony, &c. commonly called Clyſſus, are very hard to condenſe; but, by making them paſs through water, their condenſation is thoroughly effected. See fig. 6.

In the rectification of Phoſphorus, if water is made uſe of to condenſe the vapours, it will become as white as virgin wax, and almoſt as pliable; which ſeems to be owing to the water, which prevents its burning.

In the diſtillation and rectification of the Vitriolic Æther, it is of advantage to make uſe of ſpirit of wine to condenſe the vapours, which otherwiſe might have been diſſipated.

Beſides theſe, a great many other things, too tedious to mention, may be condenſed in water, or ſpirit of wine, to a very great advantage.

I cannot conclude, without mentioning that this new method of diſtillation bids fair to diſcover the mercurial and colouring earths of Beccher; for by this method we can condenſe the moſt volatile parts of all ſubſtances, far better than by any other means.

And I muſt acknowledge that I received the firſt hint of it from the common apparatus for reviving mercury from cinnabar.

I am, ſir,

Your moſt obedient ſervant,

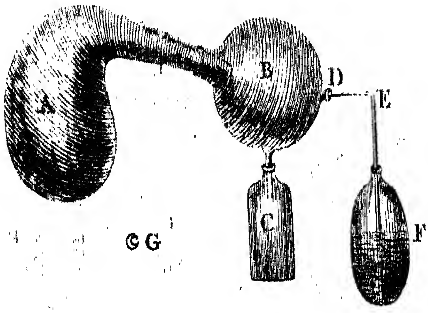
Clerkenwell,
Nov. 18, 1767.

Peter Woulfe.

To John Ellis, Eſq;
in Gray's Inn.

Expla-

Fig. 1.



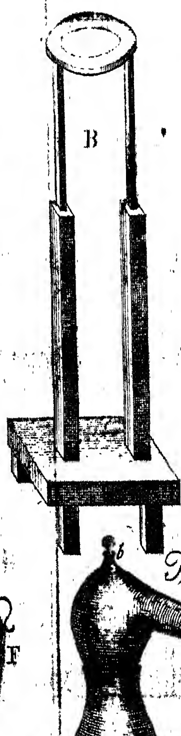


Fig. 2.

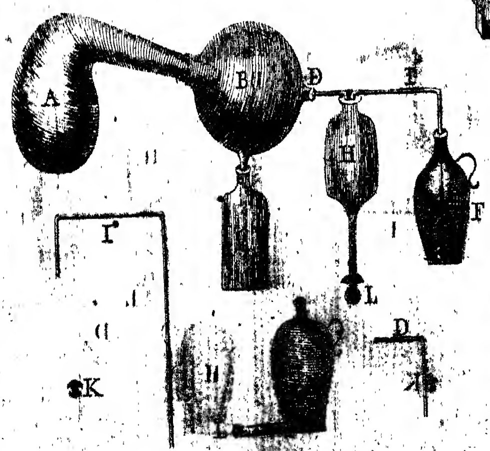
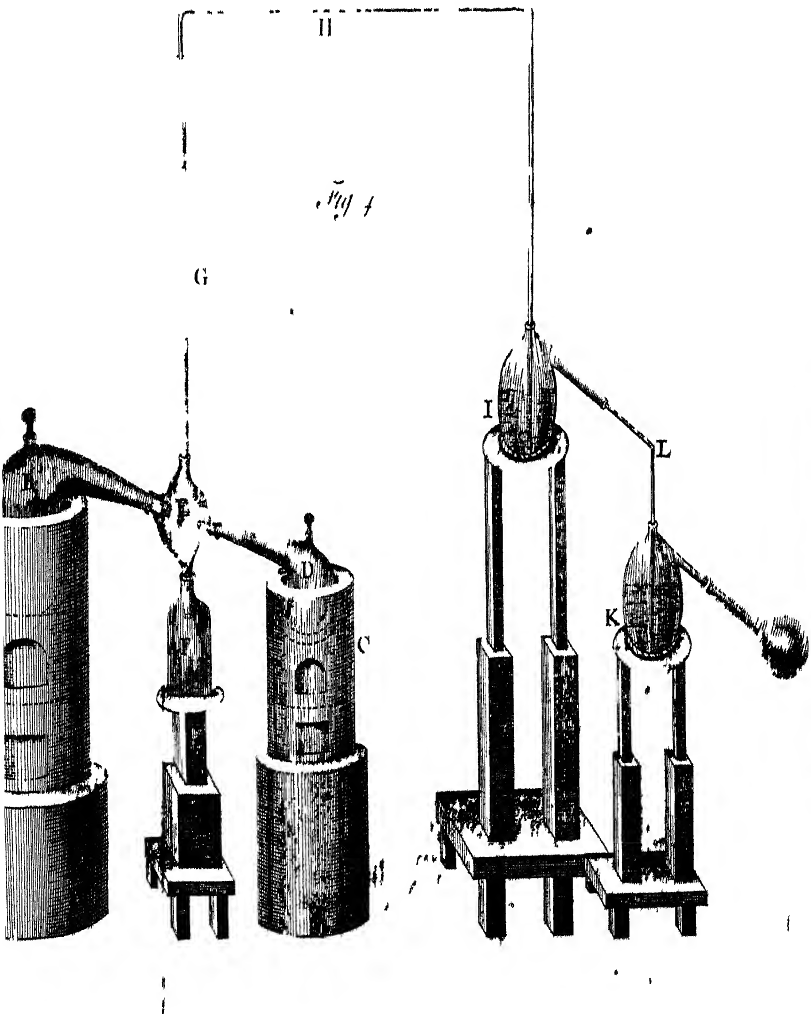


Fig. 3.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Explanation of Plate XXV.

Figure 1.

A, A glaſs retort.
B, A glaſs receiver.
C, A bottle to receive what diſtils.
F, A glaſs, or ſtone veſſel with water.
The recipient B, has a ſpout at the bottom, which conveys the liquor which diſtils into the bottle C; at the end there is a ſpout D.
E, A crooked glaſs tube $\frac{1}{10}$ and $\frac{1}{2}$ of an inch bore.
G, A cork with a ſemicircular notch to ſtop the bottle F.

Figure 2.

A, A glaſs retort.
B, A glaſs receiver.
C, A bottle to receive the diſtilled liquor.
H H, Glaſs or ſtone veſſels, with glaſs ſtoppers, ground and fitted to L L.
F, A ſtone bottle with water.
D, A crooked tube, as at E, fig. 1.
I, Another crooked tube.
K, A cork, with two ſemicircular notches to fit the crooked tubes to the veſſel H.

Figure 3.

A, An iron body with a ſtone head, which has a ſtopper at b.
B, A ſtand to ſupport the receivers and bottles.

Figure 4.

A, The furnace, in which is placed the retort B.

B, A glaſs tubulated retort, which is to be coated with loam up to B.
C, Another furnace.
D, A tubulated retort, fixed in a veſſel with ſand.
E, A ſtone veſſel, wherein the vapours of B and D are combined together.
F, A bottle to receive the liquor which diſtils.
G, A large tube fitted to E, about $\frac{3}{4}$ inch bore.
H, A crooked pipe about $\frac{1}{4}$ inch bore.
I and K, glaſs veſſels containing ſpirits of wine.
L, A crooked glaſs tube.

Plate XXVI.

Figure 5.

A, A glaſs matraſs about $4\frac{1}{2}$ feet high.
B, A glaſs head, with a ſpout and glaſs ſtopper C.
H, A glaſs tube.
P, The receiver.
E, The bottle to receive the liquor which diſtils.
F and G, Glaſs veſſels containing ſpirits of wine.
H H, crooked tubes.

Figure 6.

A, An iron or earthen retort.
B, The upper part of the retort, with an opening at top, which is to be ſtopped occaſionally.
CCCC, Crooked ſtone pipes.
DDDD, Glaſs receivers, containing water.
E, A crooked ſpout, proceeding from the laſt receivers, to let out the air that is ſet free in the operation.

INDEX.

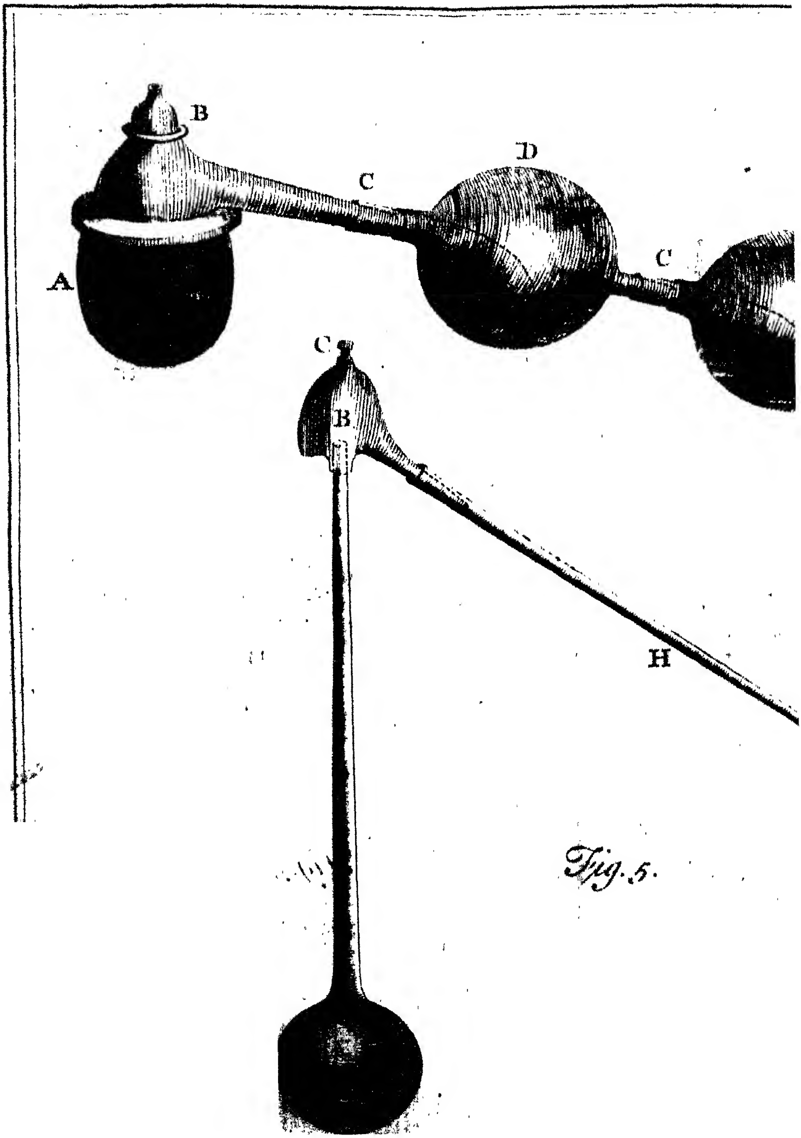
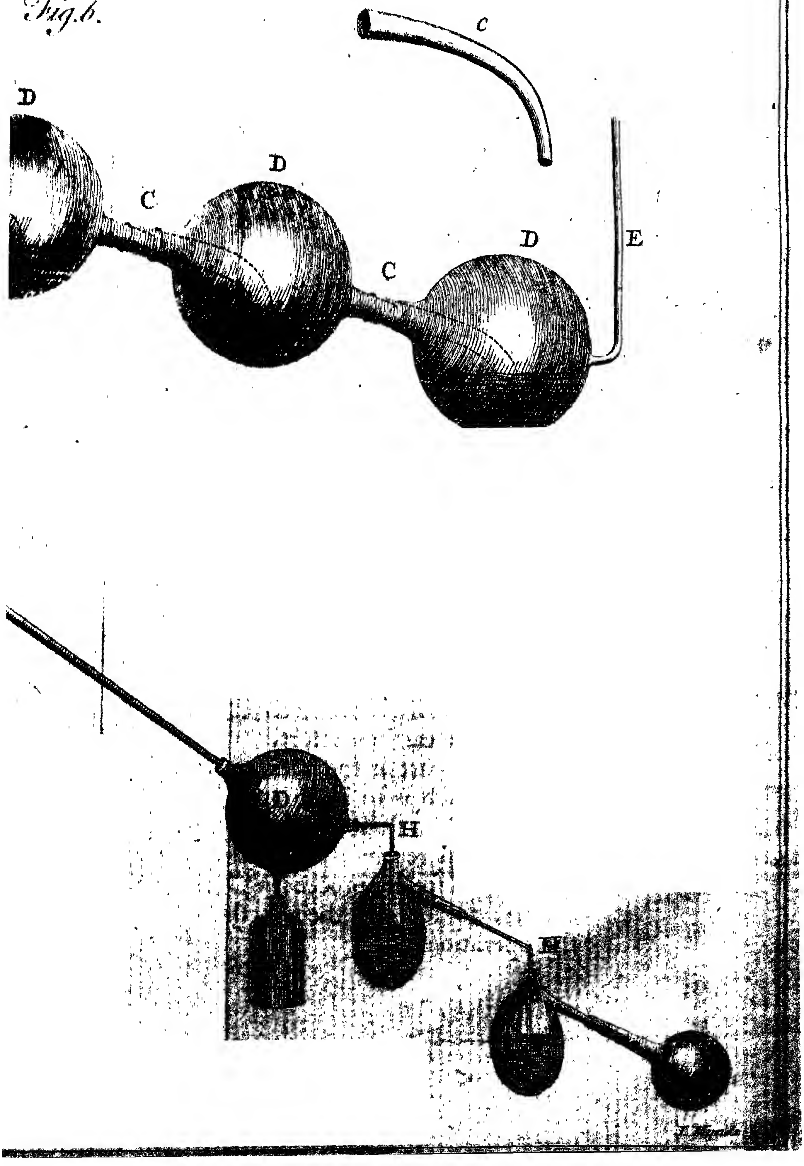


Fig. 5.

Fig. 6.



AN

INDEX

TO THE

Fifty-Seventh VOLUME

OF THE

Philosophical Transactions.

For the YEAR 1767.

A.

Atmosphere,

B.

C.

D.

E.

G.

H.

Horſley,

I.

K.

L.

M.

N

O.

P.

R.

S.

T.

V.

W.

Y.

Z.

The End of the Fifty-feventh VOLUME.

ERRATA.

Vol. LVI.

P. 6. l. 8. *for* MVRMVRIS SONI, *read* MVRMVRIS, SONI,
P. 301. *note* (9) l. 1. *for* Abulfed, *read* Abu'lfeda,
P. 301. *note* (9) l. 2. *for* Firûz, *read* Firûz

Vol. LVII.

P. 113. l. 4. *for* Wadham College *read* Wadham College,
P. 108. l. 13. *for* 1766. *read* 1765.
P. 275. l. 10. *for* Mount *read* mount.
P, 458. l. 24. *after the word* determined *insert a colon*

Date of issue.	Date of issue.	Date of issue.